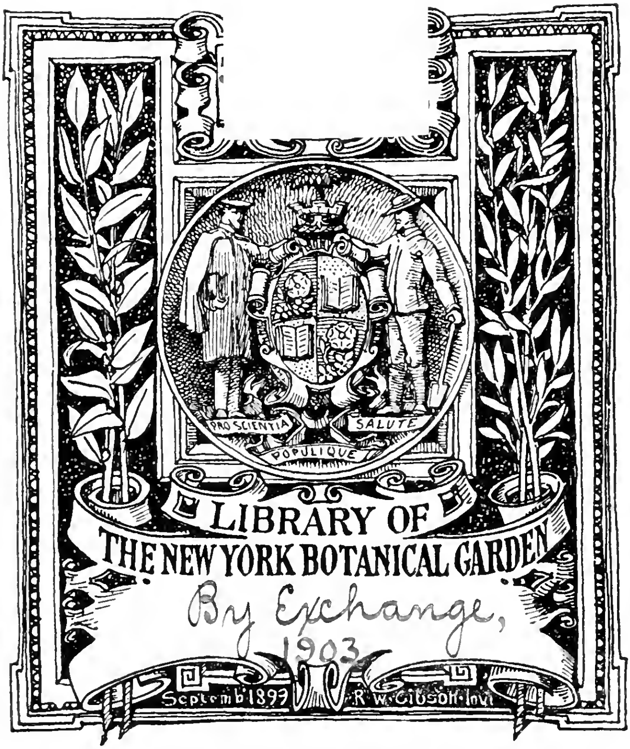
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Carnegie Institution

of

Washington

YEAR BOOK

No. 2

1903

CARNEGIE INSTITUTION

OF

WASHINGTON

YEAR BOOK

No. 2

1903

PUBLISHED BY THE INSTITUTION
WASHINGTON, U. S. A.
JANUARY, 1904

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WASHINGTON, D. C.

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BOARD OF TRUSTEES

1903–1904

OFFICERS

President of the Institution

Daniel C. Gilman

Executive Committee

Daniel C. Gilman, *Chairman*
Charles D. Walcott, *Secretary*
John S. Billings
John Hay
S. Weir Mitchell
Elihu Root
Carroll D. Wright

Finance Committee

Lyman J. Gage
Henry L. Higginson
D. O. Mills

Office of the Institution
Bond Building, Corner of New York Avenue and Fourteenth Street
Washington, D. C.

ARTICLES OF INCORPORATION

OF THE

CARNEGIE INSTITUTION OF WASHINGTON

We, the undersigned, persons of full age, and citizens of the United States, and a majority of whom are citizens of the District of Columbia, being desirous to establish and maintain, in the City of Washington, in the spirit of Washington, an institution for promoting original research in science, literature, and art, do hereby associate ourselves as a body corporate for said purpose, under An Act to establish a code of Law for the District of Columbia approved March third, nineteen hundred and one, sections 599 to 604 inclusive; and we do hereby certify in pursuance of said act as follows:

First. The name or title by which such institution shall be known in law is CARNEGIE INSTITUTION.

Second. The term for which said Institution is organized is perpetual.

Third. The particular business and objects of the Institution are the promotion of study and research, with power:

(*a*) To acquire, hold, and convey real estate and other property necessary for the purposes of the Institution as herein stated, and to establish general and special funds;

(*b*) To conduct, endow, and assist investigation in any department of science, literature, or art, and to this end to cooperate with governments, universities, colleges, technical schools, learned societies, and individuals;

(*c*) To appoint committees of experts to direct special lines of research;

(*d*) To publish and distribute documents;

(*e*) To conduct lectures;

(*f*) To hold meetings;

(*g*) To acquire and maintain a library;

(*h*) And, in general, to do and perform all things necessary to promote the objects of said Institution.

Fourth. That the affairs, funds, and property of the corporation shall be in general charge of a Board of Trustees, the number of whose members for the first year shall be twenty-seven (27), and shall not thereafter exceed thirty except by a three-fourths vote of said Board.

IN TESTIMONY WHEREOF we have hereto set our names and affixed our seals, at the City of Washington, in the District of Columbia, on the fourth day of January, 1902.

JOHN HAY [SEAL]
EDWARD D. WHITE [SEAL]
JOHN S. BILLINGS [SEAL]
DANIEL C. GILMAN [SEAL]
CHARLES D. WALCOTT [SEAL]
CARROLL D. WRIGHT [SEAL]

DISTRICT OF COLUMBIA : *ss* :

Be it remembered that on this 4th day of January, A. D. 1902, before the subscriber personally appeared the above named John Hay, Edward D. White, John S. Billings, Daniel C. Gilman, Charles D. Walcott, and Carroll D. Wright, to me personally known and known to me to be the persons whose names are subscribed to the foregoing instrument of writing, and severally and personally acknowledged the same to be their act and deed for the uses and purposes therein set forth.

Given under my hand and official seal the day and year above written.

WILLIAM MCNEIR,
[SEAL] *Notary Public.*

BY-LAWS

OF THE

CARNEGIE INSTITUTION OF WASHINGTON

1. The officers of the Board of Trustees shall be a Chairman, a Vice Chairman, and a Secretary, all of whom shall be chosen biennially, by ballot.

2. The annual meeting of the Board shall be held in Washington, on the second Tuesday of December, beginning with the year 1903. Other meetings of the Board may be called by the Executive Committee on twenty days' notice to each member of the Board, and they shall be called in the same manner by the Chairman of the Board or by the Secretary, on the written request of seven members of the Board.

3. The Trustees, by ballot, shall appoint a President of Carnegie Institution, whose term of office shall be five years. He may or may not be a member of the Board of Trustees.

4. The Trustees, by ballot, shall designate seven Trustees as an Executive Committee. Their terms of office shall be three years, and the members shall be reëligible. The Committee shall determine by lot the term of office. Any person elected to fill a vacancy shall be chosen for the unexpired part of his predecessor's term.

5. The fiscal year of the Institution shall be from November 1 to October 31, inclusive.

6. There shall be a Finance Committee consisting of three members of the Board, to be elected by the Board, to hold office until their successors are elected. The duty of such Finance Committee shall be to consider and recommend to the Board of Trustees such measures as it may believe will promote the financial interests of the Institution.

7. The Executive Committee shall, when the Board is not in session and has not given specific directions, have charge of all arrangements for administration, research, and instruction; designate advisory committees for specific duties; determine all payments and salaries; keep a written record of all their transactions and expenditures, and submit to the Board of Trustees at the annual meeting a report

which shall be published: *Provided*, That no expenditure shall be authorized or made by them except in pursuance of a previous appropriation voted by the Board.

8. The Executive Committee shall make arrangements for the custody of the funds of the Institution; keep an accurate account of all receipts and disbursements, and submit annually to the Trustees a full statement of the finances of the Institution, and a detailed estimate for the expenditures of the succeeding year.

9. In the event of a vacancy in the office of President, or in his absence or inability to perform his duties, they shall devolve upon the Secretary of the Executive Committee, who shall be a member thereof.

10. At least one month before the annual meeting, the Chairman of the Board of Trustees shall appoint a skilled public accountant to audit the accounts of the Institution.

11. The terms of all officers shall continue until their successors are elected.

12. These By Laws may be amended at any meeting of the Board of Trustees by a majority vote of the entire membership of the Board, provided written notice of the proposed amendment has been mailed to each member of the Board thirty days prior to the meeting.

MINUTES OF THIRD MEETING OF BOARD OF TRUSTEES

[Abstract]

The meeting was held in Washington, at the New Willard Hotel, on Tuesday, December 8, 1903, at 10 A. M.

The Vice Chairman, Mr. Billings, occupied the chair.

The Secretary called the roll, and the following Trustees responded to their names:

J. G. CANNON, Speaker of the House of Representatives

ALEXANDER AGASSIZ, President of the National Academy of Sciences

JOHN S. BILLINGS	WILLIAM LINDSAY
WILLIAM N. FREW	WAYNE MACVEAGH
LYMAN J. GAGE	D. O. MILLS
DANIEL C. GILMAN	S. WEIR MITCHELL
HENRY L. HIGGINSON	WILLIAM W. MORROW
E. A. HITCHCOCK	ELIHU ROOT
CHARLES L. HUTCHINSON	CHARLES D. WALCOTT

CARROLL D. WRIGHT

Absent:

THE PRESIDENT OF THE UNITED STATES

WM. P. FRYE, President of the Senate

S. P. LANGLEY, Secretary of the Smithsonian Institution

JOHN HAY	JOHN C. SPOONER
SETH LOW	ANDREW D. WHITE

Letters were received from Messrs. Frye, Hay, Low, Spooner, and White, regretting their inability to be present at the meeting.

The minutes of the second meeting were read and approved.

A resolution inviting Mr. Carnegie to attend the meeting was unanimously adopted, and in accordance therewith an invitation was sent to Mr. Carnegie, and a few minutes later he entered and remained during the session.

The Chairman announced the death of Mr. Abram S. Hewitt and Mr. William E. Dodge. Mr. Mills presented the following minute

relative to the death of Mr. Hewitt, which was read and adopted by the Board:

By the death of Hon. Abram S. Hewitt, Chairman of this Board, the Trustees are bereft of a colleague whose ability, experience, and character gave him the foremost place among those who were invited by the founder to initiate the plans of the Carnegie Institution. For these responsibilities he had exceptional qualities. A liberal education fitted him to take broad views of the needs of the country and of the progress of learning; during a long career in business he had been brought into close relations with many of the leaders in science and its applications; his services in Congress had made him familiar with the scientific departments of the government; his relations to the Cooper Union acquainted him with the aspirations and needs of promising youth; he was honored not only in the city which he served as mayor, but throughout the country, for his integrity and wisdom; in the projects of this Institution he was profoundly interested; he foresaw what good would come to the country and to the world from the judicious use of its resources. He brought to its service sagacity, enthusiasm, judgment, and strength. For these personal and official characteristics his name will be held in grateful remembrance in the annals of this foundation.

Mr. Hutchinson presented the following minute relative to the death of Hon. Wm. E. Dodge, which was read and adopted by the Board:

The first vacancy which occurred in the Board of Trustees was filled by the choice of Hon. Wm. E. Dodge. He was gratified by the appointment and in full accord with the purposes of the foundation; but soon after his election serious illness impaired his activity and his death occurred without his taking any part in our proceedings.

Mr. Dodge, like Mr. Hewitt, was a man of wide acquaintance with public affairs, who had been connected as trustee with many scientific and educational institutions, and had acquired by observation, reading, and association an acquaintance with the progress of many branches of knowledge, so that the most valuable help was expected from him.

The President of the Institution, Mr. Gilman, gave a brief review of the report of the Executive Committee, which had been printed and distributed to the Trustees in advance of the meeting.

The Secretary in his report referred principally to the financial transactions, and to the business that would come before the Trustees as resulting from the death of Messrs. Hewitt and Dodge, and the resignation of Mr. E. D. White, and the expiration of the terms of office of the officers of the Board and the members of the Executive Committee. He submitted the following financial statement and statement of assets:

CARNEGIE INSTITUTION OF WASHINGTON.

Financial Statement for the Period November 1, 1902, to October 31, 1903, Inclusive.

Receipts.		Amount.
Balance on hand November 1, 1902		$219,822 22
Received since:		
From interest on endowment	$500,000 00	
" interest on deposits with U. S. Trust Co.	5,788 09	
" interest on deposits with American Security and Trust Co.	79 01	
" Index Medicus subscriptions	2,256 91	
" sales of Year Book	29 25	
" sales of furniture	69 00	
" postage returned	27 21	
" gas deposit returned	5 36	
		508,254 83
		$728,077 05

Disbursements.			Amount.
For grants in aid of research		$135,617 02	
For publications		959 28	
For administration:			
Trustees' expenses	$950 14		
Executive Committee's expenses	1,014 46		
Honorariums and expenses of Advisers and Secretary	21,226 71		
Salaries of officers	13,000 00		
Pay of employés	2,692 23		
Rent, fuel, and light	2,564 18		
Furniture	1,235 28		
Stationery and office supplies	302 89		
Printing	1,722 90		
Postage, express, and telegrams	408 93		
Telephone	62 55		
Miscellaneous	373 79		
		45,554 06	
For purchase of bonds for reserve fund		100,475 00	
			$282,605 36
Balance on hand October 31, 1903:			
On deposit with U. S. Trust Co.		$445,135 31	
On deposit with American Security and Trust Co.		336 38	
			445,471 69
			$728,077 05

Assets of the Carnegie Institution.

In the endowment:		
One hundred registered 5 per cent. bonds of the United States Steel Corporation		$10,000,000
In the reserve fund:		
Northern Pacific Railway Company Land Grant General Mortgage 4's of 1997	$50,000	
Atchison, Topeka, and Santa Fe Railroad Company General 4's of 1995	50,000	
		100,000
Office outfit at Washington (estimated)		1,500
		$10,101,500

On the submission of the report of the Executive Committee, the chairman made a general statement supplementary to that made by the President, in relation to the results of the work accomplished by the Institution thus far, and of the scope of its future work as indicated by the grants recommended.

A somewhat extended discussion followed on the subject of minor and larger grants, and a series of resolutions was passed making the following general appropriations:

Reserve fund	$100,000
Publication fund, to be continuously available	40,000
Administration	60,000
Grants for large projects	130,000
Grants for minor researches	200,000
Special grants	43,000

Mr. Carnegie then expressed his satisfaction with the progress thus far made, and with the plans as outlined for the future, and thanked the Trustees for the work they are doing for the Institution.

At 12.30 the Board took a recess until 2 P. M.

At the second session there were present:

Prof. ALEXANDER AGASSIZ, President of the National Academy of Sciences

JOHN S. BILLINGS	WILLIAM LINDSAY
WILLIAM N. FREW	D. O. MILLS
LYMAN J. GAGE	S. WEIR MITCHELL
DANIEL C. GILMAN	WILLIAM W. MORROW
HENRY L. HIGGINSON	ELIHU ROOT
E. A. HITCHCOCK	CHARLES D. WALCOTT
CHARLES L. HUTCHINSON	CARROLL D. WRIGHT

The Board took up the question of a seal for the Institution, and referred the matter to the Executive Committee with power.

Amendments to the By Laws were then considered. By Law No. 10 was amended by substituting in the second line the word "skilled" instead of "authorized."

The Board then proceeded to fill the vacancies caused by the death of Mr. Hewitt and Mr. Dodge, and the resignation of Mr. E. D. White. The election resulted in the choice of John L. Cadwalader, New York; Cleveland H. Dodge, New York; and Wm. Wirt Howe, New Orleans.

The election of members of the Executive Committee to fill the places of Messrs. Gilman, Mitchell, and Wright, whose terms expired, resulted in their reëlection to the class of 1906.

Mr. John Hay was elected to succeed Mr. Hewitt on the Executive Committee in the class of 1905.

The Board then elected its officers to fill the vacancies caused by the death of Mr. Hewitt and by the expiration of terms of office of Vice Chairman J. S. Billings and Secretary C. D. Walcott as follows:

Chairman, J. S. Billings.

Vice Chairman, Elihu Root.

Secretary, C. D. Walcott.

The report of the Finance Committee was then read and approved.

At 3.10 P. M. the Board adjourned.

CHAS. D. WALCOTT,
Secretary.

REPORT OF EXECUTIVE COMMITTEE ON THE WORK OF THE YEAR

Introduction.

Immediately after the Board of Trustees adjourned, November 25, 1902, the Executive Committee began to consider its various directions, and also such matters as had been recommended by the Committee and approved by the Board.

During the fiscal year the Executive Committee held nine meetings.

Its organization continued the same as last year—Mr. Gilman, Chairman, and Mr. Walcott, Secretary. Mr. Marcus Baker continued as Assistant Secretary, taking charge of editing, printing, accounting, and the general direction of all routine work in the office.

Grants Made and Reports Thereon.

At the last annual meeting the Trustees set apart $200,000 for grants for research during the fiscal year 1902–1903. The following is a list of grants made by the Executive Committee under such authority. Each one is accompanied by a brief statement of the results thus far obtained. When an investigation is completed, a final report will be submitted by the grantee. This may be printed either in abstract or in full in the Year Book.

Anthropology.

G. A. Dorsey, Field Columbian Museum, Chicago, Ill. Grant No. 43. *For ethnological investigation among the Pawnees.* $2,500.

Abstract of Report.—This scheme of investigation will require four or five years for its completion. It is a study of the religious ceremonies of the Pawnee Indians, with direct reference to the mythological origin of each ceremony, and to obtaining a clear and comprehensive understanding of the religious systems of the Pawnees.

The work of collecting and arranging the details of the region of the religion was begun early in the year, and has been pushed forward as rapidly as possible. The work of the first year was to obtain the mythology of the Skidi on the one hand, and the Chaui, Kitkahahki, and Pittahauirata bands of Pawnees on the other, and of the Wichita and Arikara. The second result sought for was to gain a comprehensive insight into all the ceremonies of the four bands of the Pawnees and of the Arikara. Of these two results as much has been achieved as could be hoped for, inasmuch as the work has progressed for only about nine months.

With the beginning of the first of the Skidi ceremonies early next spring, it will be possible to select certain of the more important ones for more detailed observations. Thereafter each ceremony will be studied independently and in detail, and the observations thus made, together with the ritual as sung, will be prepared for publication.

Wm. H. Holmes, Director Bureau of American Ethnology, Washington, D. C. Grant No. 44. *For obtaining evidence relative to the early history of man in America.* $2,000.

The phenomena to be considered are scattered and obscure. The geological formations of both continents, ranging from Eocene to Recent, abound in various records, but investigation has been in the main desultory and unscientific, and the isolated observations are to-day without adequate correlation.

Mr. Holmes proposed to begin his work with the compilation of all data respecting previous investigations, and then to begin field work which should extend to deposits in caves and caverns where men have lived, and should also include their ancient sites, such as kitchenmiddens, shell heaps, and earthworks.

Abstract of Report.—The field work in this investigation was done mainly by Mr. Gerard Fowke, archeologist, who began work in Indiana and carried his examinations into Illinois, Kentucky, Tennessee, and Alabama, exploiting many caves and making careful investigation of a few. Results were distinctly negative with reference to the principal question at issue, the entire season's work having developed no fact that will tend to establish a theory of the great antiquity of man in America. The season's work, however, was not a failure on this account, since the question is one that must be solved, if not by the discovery of positive evidence, by establishing the universality of negative evidence.

Late in the season explorations were begun on the Atlantic slope by Mr. F. B. McGuire, archeologist, in the caves of the upper Potomac in West Virginia. Mr. Holmes personally made a reconnaissance in Georgia and Alabama for the purpose of collecting definite information regarding the caves of the south.

With the aid of Mr. F. B. McGuire and Dr. J. W. Fewkes, a cave in Porto Rico was explored without expense to the Institution.

The present report can be regarded as only one of progress, since Dr. Fewkes and Mr. McGuire are still in the field.

George F. Kunz, New York city. Grant No. 52. *To investigate the precious stones and minerals used in ancient Babylonia in connection with the investigation of Mr. William Hayes Ward.* $500.

Abstract of Report.—This is an investigation in coöperation with that of Mr. William Hayes Ward. It was deferred until winter in order to secure the coöperation of Mr. Ward after his return from his investigations in Europe.

William Hayes Ward, New York city. Grant No. 50. *For study of oriental art recorded on seals, etc., from western Asia.* $1,500.

Dr. Ward has been for fifteen years devoting his spare time to oriental archeology, with special reference to the beginnings of art and mythology, as shown in recovered monuments and especially in the seal cylinders, which preserve a large part of the early art. He has handled thousands of seals and has paper impressions of thousands. The investigation covers a period from about 4000 B. C. to about 400 A. D. and will include a study of the mythological representations and various designs, emblems, and inscriptions contained in them.

Abstract of Report.—During last summer Dr. Ward visited various museums in the United States and in Europe, where he examined the great collections of Paris and Berlin. Every facility was granted by the authorities in charge, and he made notes and obtained casts of such cylinders and seals as were required for his investigations. He is now engaged in the preparation of manuscript and illustrations. It is estimated that it will require about two years to complete the study and prepare the results for publication.

Astronomy.

Lewis Boss, Dudley Observatory, Albany, N. Y. Grant No. 7.
For astronomical observations and computations. $5,000.

Abstract of Report.—This work has for its ultimate object an investigation upon the motions of the brighter stars (all down to the seventh magnitude), and of all stars, of whatever magnitude, supposed to have motions as great as 10″ per century, and of many other stars which were specially well determined prior to 1850.

During the year Professor Boss's attention was given to—

(*a*) The compilation for each star of all observations for position that have been made upon it during the history of astronomy. Some stars are found in more than sixty catalogues.

(*b*) Investigation of the systematic errors with which each series of meridian observations seems to be affected, in order that the precision of the results may be notably increased. This involves in the first place the establishment of a standard of reference, which must include the positions of all those stars which have been most frequently and accurately observed.

The entire work is proceeding upon a logical plan carefully studied and formulated through the results of experience during past years, with a view to economy in the succession of individual investigations designed to contribute to the final result. In an extensive investigation of this kind there is always an element of danger. If the work is so planned that definite results cannot be realized until the completion of the whole work, there is liability to serious loss from the ordinary accidents of life which cannot be foreseen. Therefore this work has been so planned that useful results can be secured and promptly published at every successive stage of the work. Each step grows logically out of those which have preceded it. The computations are so planned that successive improvements in the fundamental basis can be introduced with the least possible duplication of work.

It is intended that the catalogue of more than 2,500 standard stars shall be offered for publication to the Carnegie Institution early in 1905, and if no unforeseen accidents occur this program should be entirely feasible.

During the present year the catalogue of 627 standard stars has been passing through the press and is now nearly ready for issue. Subsidiary investigations connected with this catalogue have been carried out under the grant of the Institution for this year.

Boss, Hale, and Campbell. Grant No. 70. *For investigating proposal for a southern and a solar observatory.* $5,000.

In the Year Book for 1902 a proposition for the establishment of a distinctly solar observatory was presented by Professor S. P. Langley. In the same report (page 89) the astronomical advisers called attention to the lack of observatories in the southern hemisphere, and in an appendix (pages 99 to 104) they treated the subject still more fully.

In order that the Board of Trustees might be enabled to arrive at appropriate conclusions, Professor Lewis Boss, chairman; Professor George E. Hale; and Professor W. W. Campbell were requested to investigate, as a Committee, the subject more fully and to consider the question of suitable sites for such observatories.

The result of the work of this committee is submitted in the Accompanying Papers of this Report, pages 1–70.

W. W. Campbell, Lick Observatory, Mt. Hamilton, Cal. Grant No. 53. *For pay of assistants to take part in researches at the Lick Observatory.* $4,000.

Abstract of Report.—Owing to the difficulty of obtaining satisfactory assistants from the east and providing living quarters for them on the mountain, it was not found possible to provide for an effective use of the grant for the employment of assistants and computers until late in the year.

Investigations were begun with the meridian circle work and in spectroscopy. With the construction of additional residence quarters on the mountain, Professor Campbell will soon employ the full number of assistants rendered possible by the grant.

Herman S. Davis, Gaithersburg, Md. Grant No. 11. *For a new reduction of Piazzi's star observations.* $500.

American and European astronomers have urged that a fresh reduction of these observations by known methods for obviating certain errors should be made. Professor Porro, of Turin, undertook a part of the reductions and Professor Davis the rest. Assistance from private persons and from observatories have contributed to the prosecution of this undertaking. The Carnegie Institution was asked to make a small contribution.

Abstract of Report.—The work accomplished under this grant has been in connection with work that was already begun. This makes

it difficult to define specifically the exact amount done under the grant from the Carnegie Institution. The period of nine months, during which the grant has been available, has marked the transition from the routine work of reducing the observed "apparent" positions of the stars to a common "mean" epoch to the next large step of deducing therefrom the instrumental errors and compiling the final catalogue. This rendered it necessary to spend this time in rounding out and perfecting all the divers portions of the computations which have been going on uninterruptedly for the past seven years. This has been finished, and also some preliminary work done for the next great and distinct stage of the work:

(*a*) To deduce the errors of the telescope for each night of observation.

(*b*) To correct all observations for these maladjustments.

(*c*) Finally, to combine the definite separate positions into means for each star included in the catalogue, which is the goal of the long labor.

George E. Hale, Yerkes Observatory, Williams Bay, Wis. Grant No. 13. *For measurements of stellar parallaxes, solar photographs, etc.* $4,000.

Abstract of Report.—Work was begun on the photographic investigation of stellar parallaxes early in May with a 40-inch telescope. Up to October, 114 plates, containing about 350 exposures, had been obtained. These included:

(*a*) Twenty experimental plates.

(*b*) Eighty-eight plates suitable for parallax determinations.

(*c*) Six plates of loose star clusters.

Considerable work was also done in the measurement of photographs of star clusters.

Another line of investigation was the photometric determination of stellar magnitudes. Considerable progress was made in this, fields being measured with the 6-inch reflectors and the 12 and 40-inch refractors. Measures were also made upon the Pleiades group of stars to determine the constant of the equalizing wedge photometer. Measurements were also made of comparison stars for faint variables.

Much progress was also made in the measurement and discussion of photographs of the sun, taken with the spectroheliograph at the Kenwood Observatory in the years 1892–1896, and in other minor investigations connected with the work in hand.

Simon Newcomb, Washington, D. C. Grant No. 17. *For determining the elements of the moon's motion and testing the law of gravity.* $3,000.

Much of the material for this investigation, consisting of computations of places of the moon from Hansen's tables and their comparison with observations, was preserved in the archives of the Nautical Almanac Office, waiting an opportunity for their working up. By permission of the Secretary of the Navy, Hon. William H. Moody, these papers were entrusted to the Carnegie Institution and by the Institution to Professor Newcomb.

Abstract of Report.—The importance of this work grows out of the fact that new tables of the moon are urgently required for the purposes of astronomy and of navigation. For a long period the problem of constructing and perfecting such tables has been delayed by an unexplained discordance between the observed motion of the moon and the motion which should result from the action of all known bodies upon it. The exact cause of this discordance cannot be recorded, because the observations from 1750 to 1850 have never been worked up and compared with the tables. The problem of determining the exact nature of the deviation of the moon from its predicted place is twofold. The observations since 1750 must be worked up, and in order to compute the comparison the action of the planets on the moon must be recomputed with a view to determining whether any correction to the past computations is necessary.

By aid of a grant from the Carnegie Institution an important term of long period, produced by the action of Venus, has been recomputed.

Professor Newcomb has taken up the work on the adopted plan of the occultations of stars by the moon, a work that he had begun in connection with the Nautical Almanac. This, in connection with the incorporation of other important observations, can probably be completed in two years more.

E. C. Pickering, Harvard University, Cambridge, Mass. Grant No. 20. *For study of the astronomical photographs in the collection of Harvard University.* $2,500.

Abstract of Report.—The grant made to Professor Pickering was applied to a great variety of uses. These included sums paid to 19 different assistants and computers, and for other assistance in connection with the Harvard Observatory.

Each of the numerous investigations is of importance in carrying forward the work going on in the observatory, but they do not appear to be upon sufficiently definite and specific problems, as given in his report, to permit of a distinct statement, in most cases, of the progress of the work under the Carnegie Institution grant.

Professor Pickering reports that in forming a corps of observers to study the photographs, time and money being limited, it was difficult to decide what subjects to select from this vast amount of material. A number of problems have accordingly been studied which serve to illustrate the various investigations which might be undertaken. Abridged results of a portion of these were promptly published in the Harvard Observatory Circulars Nos. 69 and 70. The principal researches carried on are as follows:

1. Eclipses of Jupiter's satellites.
2. Light curves of Algol variables.
3. Position and brightness of stars in clusters.
4. Observations have been made of the changes in light of 9 variable stars of long period, during several years before they were discovered.
5. Early observations of stars of the Algol type and other variables of short period.
6. Transit photometer.
7. Nova Geminorum.
8. Variations in brightness of Eros.
9. Proper motion of stars.
10. Missing asteroids.
11. Many images of interesting objects like new stars, variables, and asteroids doubtless appear on the photographs. An examination has accordingly been made of several of the plates to determine whether it would be advisable to examine a large number of them systematically for the discovery of such objects.

Wm. M. Reed, Princeton Observatory, Princeton, N. J. Grant No. 54. *For pay of two assistants to observe variable stars.* $1,000.

Abstract of Report.—Owing to the difficulty of obtaining an observer, work was not begun till March 1. During the seven months from March 1 to October 1, the 23-inch telescope of the Halsted Observatory, exclusively for photometric work, was used on every clear night from early in the evening until daylight. In all, 9,015 observations were made on about 50 different stars.

Three classes of stars were observed:

(*a*) Such variable stars as are too faint to be reached by any except the largest telescopes. In particular, selection was made of stars that have become too faint for the Harvard observers and those coöperating with them.

(*b*) Measurement of faint stars that are to be used as standards of magnitude. In this work they are connecting stars of the 13th magnitude with those of the 15th magnitude. The Lick and Yerkes observatories are connecting the 15th magnitude stars with the 16th magnitude, and the Harvard Observatory is connecting the 11th magnitude with the 13th magnitude.

(*c*) A special study of the newly discovered Algol variable, 4.1903 Draconis, has been made, and a preliminary article giving the results of these observations has been sent to the *Astronomical Journal*.

Mary W. Whitney, Vassar College, Poughkeepsie, N. Y. Grant No. 23. *For measurement of astronomical photographs, etc.* $1,000.

Abstract of Report.—This work consists in the measurement and reduction of stellar photographs taken at the observatory at Helsingfors, Finland, by Professor Donner. The measurement of the eight plates is finished and the reduction is well along. A preliminary catalogue of the mean places of 404 stars within two degrees of the pole is nearly completed. The work was pressed during the last quarter, as Professor Whitney then secured the services of an expert computer. The intercomparison of the plates and the determination of proper motion remains to be studied.

BIBLIOGRAPHY.

Robert Fletcher, Army Medical Museum, Washington, D. C. Grant No. 30. *For preparing and publishing the Index Medicus.* $10,000.

The Index Medicus was established in 1879, under the direction of Dr. John S. Billings and Dr. Robert Fletcher, and discontinued in 1899, after 21 volumes had appeared, for the lack of pecuniary support.

Abstract of Report.—The scope of this work is very broad with relation to the medical sciences. It contains, in classified form, month by month, reference to everything published throughout the world which relates to medicine or public hygiene. The latter comprises

all that concerns the public health in its municipal, national, and international relations.

Nine numbers of the volume have been issued, and the volume will be complete with the January number, when the "Annual Index" will be compiled. The Index is a very elaborate piece of work, and will comprise 200 pages in double or triple columns. The work is of great value to all the medical profession, especially to professors in medical schools and colleges, officers of health, and workers in scientific laboratories.

The subscribers to the Index Medicus are chiefly residents of the United States, but the list includes subscribers in England, Ireland, Scotland, Canada, Australia, France, Germany, Spain, Portugal, Roumania, Sweden, Switzerland, and Manila. There are now 455 subscribers.

Herbert Putnam, Washington, D. C. Grant No. 56. *For preparing and publishing a Handbook of Learned Societies.* $5,000.

In order that the scientific investigators of this country, and especially those connected with the Carnegie Institution, might have an accurate knowledge of the agencies which now exist for the promotion of scientific inquiry in every part of the world, the Advisory Committee on Bibliography recommended that a descriptive catalogue be prepared of all the learned societies of the world.

At the present time such information, and particularly regarding the publications of learned societies, is incomplete and unorganized, being scattered through a large and miscellaneous collection of volumes, many of which are inaccessible and not well known. A careful and comprehensive list would be of great value to all the librarians of the country who aim at the preservation of the transactions of learned bodies. It would also furnish a basis for exchanges. The funds for research work held by these various institutions have special significance with reference to the activities of the Carnegie Institution. The plan of the Handbook included information as to these eleven points:

(1) Name or names of the society or institution, indicating any change which may have occurred, with cross references.

(2) Objects of the society.

(3) Brief historical note.

(4) Endowments, research funds, prizes, etc.

(5) Offices of the society.

(6) Membership, numbers, conditions and manner of election, dues, etc.

(7) Meetings—their character, frequency, time, and place.

(8) Communications—regulations for presentation and publication of papers.

(9) List of officers, with address of corresponding secretary.

(10) Complete and detailed bibliography of all regular or special publications since the foundation of the society, editions (how large?) to satisfy all the above mentioned requirements.

(11) Publications—conditions and methods of distribution; prices.

According to the plan of work approved, the handbook is to be in volumes; societies to be classified by subjects, with local arrangement, and each class to constitute a separate part. The following order of procedure has been adopted:

(*a*) To prepare a list of societies from the exchange lists at the Smithsonian Institution and elsewhere in Washington, and a card catalogue to keep orderly record of communications.

(*b*) To issue a suitable circular to these societies, requesting the desired information.

(*c*) To prepare for publication the material received, filling out lacunæ by further correspondence and reference to various sources of information.

(*d*) In the case of societies not replying to circular or letter, and in regard to which sufficient information cannot be obtained from printed sources, to adopt such other methods as the progress of the work may suggest.

The first stage of this work was the preparing of a card catalogue of names of learned societies and institutions. Every source of information known and available in the Congressional Library was searched to make this as nearly complete as possible, at the same time separating (1) dead societies and (2) societies not publishing any material of importance to investigators.

The second stage of the work was the sending of a circular letter, containing an outline of the information required, to academies and societies dealing with historical and social science in Europe and North America. Russia and other Slavic countries, and also Austria and Hungary, are being treated independently, advantage being taken of a visit to Russia by Mr. A. V. Babine, of the Library of Congress. Mr. Thompson and Mrs. Thompson made personal visits to England, Paris, Belgium, Holland, and Berlin for the purpose of

supplementing the information obtained by correspondence. It is anticipated that Mr. Thompson will also visit Italy and Switzerland.

The third stage of the work, the reduction of the replies received to standard form, was begun in August, and is now going on in the office at Washington. It is expected that this work will be brought to completion in 1904.

BOTANY.

W. A. Cannon, New York Botanical Garden, N. Y. Grant No. 27. *For investigation of plant hybrids.* $500.

Abstract of Report.—Under this grant Mr. Cannon worked at the New York Botanical Garden until September 1, 1903. He prepared a paper on the spermatogenesis of the hybrid peas* and collected material for the study of the sporogenesis of two fern hybrids.

H. S. Conard, University of Pennsylvania, Philadelphia. Grant No. 8. *For study of types of waterlilies in European herbaria.* $300.

Abstract of Report.—The grant made to Mr. Conard was to enable him to examine the types of waterlilies in various European herbaria for the purpose of completing a memoir on waterlilies which the Carnegie Institution is about to publish. He was successful in obtaining the requisite data, and the memoir will soon go to press.

Desert Botanical Laboratory (F. V. Coville and D. T. MacDougal, Washington, D. C.). Grant No. 26. $8,000.

At the meeting of the Trustees in November, 1902, a comprehensive plan for the encouragement of botanical researches was submitted by the Advisory Committee on Botany (see Year Book, No. 1, pages 3–12).

In carrying out this plan, Mr. F. V. Coville, Botanist of the Department of Agriculture, Washington, and Mr. D. T. MacDougal, Director of the Laboratories of the New York Botanical Garden, were requested to go to the arid lands of the west and make such further recommendations as might seem to them best. They became persuaded that the best position for the laboratory, considering both natural and artificial advantages, is Tucson, Arizona, and they recommended its establishment there and the engagement of Dr. W. A. Cannon to be resident investigator.

*Contributions of New York Botanical Garden, No. 45, Bull. Torrey Bot. Club, Oct. 30, 1903.

A full report with respect to the organization of this laboratory and of the various circumstances which led up to it will be published in a monograph soon to be printed among the publications of the Carnegie Institution.

Abstract of Report.—Messrs. Coville and MacDougal were appointed a committee on the subject of a Desert Botanical Laboratory.

After their visit to the principal points in the southwestern desert region, a laboratory location was selected near Tucson, Arizona.

The building site, water supply, road, and electrical connection were presented by the Chamber of Commerce of Tucson, the cash value of these concessions amounting to about $1,400, and the discussions that took place initiating what is still more valuable—the hearty interest and coöperation of the citizens in the purposes of the laboratory.

A laboratory building has been planned, contracted for, and completed, the contract price being $3,843. The laboratory has been equipped with books, apparatus, furniture, and supplies, at a cost of $1,813.50.

Dr. W. A. Cannon, recently connected with the New York Botanical Garden (Bronx park), New York, was appointed resident investigator, and took charge of the laboratory September 1. He is now engaged in investigating the root systems of desert plants with reference to their special devices for the absorption and storage of water.

The privileges of the laboratory have been granted to Professor Charles B. Davenport, University of Chicago, for an inquiry into the morphological and physiological adjustments of desert animals to their habitat. Other applications are pending.

The committee has presented an illustrated report on the laboratory location, which is now in press as a publication of the Institution.

E. W. Olive, Crawfordsville, Ind. Grant No. 32. *Researches on the cytological relations of the Amœbæ, Acrasiæ, and Myxomycetes.* $1,000.

Abstract of Report.—Mr. Olive's work has been carried on in Professor Strasburger's laboratory in the Botanical Institute at Bonn, Germany. In order to do this work he resigned his position as instructor at Harvard University. His studies include cultures of the Acrasiæ and of the Labyrinthuleæ, which he had brought from America.

Mr. Olive's report shows definite progress in his research, and the prospect of the completion within two months of two papers incorporating a portion of his results.

Janet Perkins, working at the Royal Botanical Gardens, Berlin, Germany. Grant No. 19. *For preliminary studies on the Philippine flora.* $1,900.

Abstract of Report.— Dr. Janet Perkins reports that she was engaged in the proposed investigation from February 20 to October 5, 1903. A catalogue of the Philippine flora was begun, based on various monographs and papers which have appeared in scientific periodicals. This work consumed much time, as literature regarding the Philippines is greatly scattered, and the synonymy needs a thorough clearing up.

Among other matters that were begun were :

(*a*) A catalogue of the various native names.

(*b*) A list of botanical literature pertaining to the Philippines.

(*c*) The attempt to construct a type herbarium of Philippine plants.

(*d*) The determination of certain Philippine plants received from the Department of Agriculture.

(*e*) The preparation of a sample copy of the manuscript and illustrations for the position of the family Marantaceæ.

Chemistry.

John J. Abel, Johns Hopkins University, Baltimore, Md. Grant No. 24. *For study of the chemical composition of the supra-renal gland.* $1,000.

Abstract of Report.—The vital importance of the supra-renal glands was first pointed out a half century ago. Repeated efforts to explain why these organs are physiologically necessary were of no avail until 1895, when it was discovered that the juice expressed from these glands contains a remarkable principle found nowhere else in the human body.

This important substance has the power to raise the blood-pressure of man and other animals when given in very minute amounts,

and this fact has been made the basis of a theory which seeks to explain the importance of these glands, which alone secrete it.

It is evident that but little scientific or practical progress could be made until this active principle should be separated from the innumerable other constituents of the gland. This isolation was first effected by Dr. Abel in 1897, and he named the product obtained by him Epinephrin.

Investigators are making every effort to elucidate the chemical constitution of this compound, not only with a view to tracing its origin and its fate in the animal body, but also looking to its possible preparation by synthetic methods—that is, by chemical art, without the intervention of the animal organism.

Progress of the most promising character has recently been made in Dr. Abel's laboratory. The work is being actively prosecuted and will be continued throughout the present winter.

Since receiving a grant from the Carnegie Institution, Dr. Abel has published three papers on the subject, and a fourth communication bearing on the true composition of epinephrin and epinephrin hydrate and their chemical constitution will shortly make its appearance.

W. D. Bancroft, Cornell University, Ithaca, N. Y. Grant No. 6.
For a systematic chemical study of alloys, beginning with the bronzes and brasses. $500.

Abstract of Report.—The experimental work under this grant has been done by Mr. E. S. Shepherd, under the direction of Professor Bancroft. They have analyzed the different solid bases and determined the copper-tin-lead diagram except for the alloys containing less than 20 per cent of copper. They have determined the densities and electro-motive forces of the annealed bronze, and made a careful microscopic study of the same alloys. Work is now under way on the density and determination of bronzes cast in vacuo, the copper-tin-lead diagram, and the making of the necessary analyses. A study of the physical properties of bronzes will be carried on during the winter.

L. M. Dennis, Cornell University, Ithaca, N. Y. Grant No. 42.
For investigation of the rare earths. $1,000.

Professor Dennis has been engaged for the past ten years in the study of the rare earths, and has accumulated a large amount of purified material. He proposed to carry on a study with special reference to improvements in the methods for determining the atomic masses of these substances, and for separating the elements of the yttrium group.

Abstract of Report.—The work under this grant was carried on by Dr. Benton Dales in the laboratory of Professor Dennis, of Cornell University.

Dr. Dales has submitted a report on the ammonium carbonate and acetic acid method of fractionation. The source of the rare earths used in the work was xenotime, essentially a phosphate of the yttrium group of earths from Brazil. The work is unfinished, owing to Dr. Dales having resigned his position at Cornell University before completing it. Three-fourths of the grant was used. A paper containing the results of the investigation, as far as obtained, was transmitted for publication.

H. C. Jones, Johns Hopkins University, Baltimore, Md. Grant No. 39. *For investigations in physical chemistry.* $1,000.

Abstract of Report.—Under the direction of Professor Jones, Dr. F. H. Gatman began work October 1, 1903, by investigating certain apparently abnormal phenomena manifested by concentrated solutions of electrolytes in water and other solvents. They expect to be able to report considerable progress by the end of the year.

H. N. Morse, Johns Hopkins University, Baltimore, Md. Grant No. 34. *For researches on osmotic pressure.* $1,500.

Abstract of Report.—Professor Morse reports that the immediate problem to be solved was the development of a practical method for measuring osmotic pressure. Although osmotic pressure has been recognized for twenty five years as one of the great forces of nature, there have been no direct measurements to furnish an adequate experimental basis for the laws supposed to govern it. Professor Morse has been engaged for several years in attempting to overcome the difficulties which lie in the way of quantitative measurements of osmotic pressure. He states the problem under three heads, as follows:

1. The preparation of a suitable semipermeable membrane.

2. The overcoming of the mechanical difficulties in assembling the different parts essential to the complete osmotic cell.

3. The production of an efficient porous wall on which to deposit the semipermeable membrane.

Professor Morse has succeeded in solving the problems designated by 1 and 2, and the work since October, 1902, has been prosecuted by him and Mr. J.C.W. Fraser, working in the laboratory of the Johns Hopkins University. They have found it necessary not only to work out theoretically but also practically the problem of the production of a suitable porous wall, necessitating the molding of the clay under great pressure in order to give the cell wall a higher and more uniform degree of compactness than is secured by the usual methods of the potter, and to remove thoroughly the air blisters and cavities which render most porous walls unfit for experimental work in osmotic pressure. Their attention was therefore turned, in the second place, to the devising of apparatus for the forming of the clay vessels under pressure, with the result that they now possess two pieces of apparatus which work to entire satisfaction. They next proceeded to take up the problem of baking the clay vessels, and devised an electric kiln which was effective and well adapted to general use in the laboratory. They are now ready to begin the making, baking, and burning of porous cells.

A. A. Noyes, Massachusetts Institute of Technology, Boston, Mass.
Grant No. 45. *For certain chemical investigations.* $2,000.

Abstract of Report.—The work under the direction of Professor Noyes, on the electric conductivity of salts and aqueous solutions at high temperatures, has been in progress for several months, with the assistance of Dr. William D. Coolidge. Much of the time has been given to the construction of an effective platinum lined conductivity cell or bomb, suitable for exact conductivity measurements with aqueous solutions up to 306° or higher, and in other preparatory work.

Now that the serious difficulties in the production of the conductivity apparatus, suitable for measurements at high temperatures and pressures, have been overcome, and the possibility of obtaining accurate results has been demonstrated by a series of determinations extending with a few salts up to 306°, it is highly desirable to extend the measurements to salts of other types and to acids and bases, and to the critical temperature of 360°. This work is very difficult,

and it will be necessary to continue it for a number of years before it will be completed.

Two other researches for which the aid granted was employed were begun in September, with the assistance of Dr. Herman C. Cooper and Mr. Yogoro Kato.

Theo. W. Richards, Harvard University. Grant No. 41. *For investigation of values of atomic weights, etc.* $2,500.

Abstract of Report.—Professor Richards has submitted a memoir, about to be published by the Carnegie Institution, containing the records of his experiments on a new method of determining compressibility. By means of this method the compressibilities of bromine, iodine, chloroform, bromoform, carbon tetrachloride, phosphorus, water, and glass have been determined over a range of 700 atmospheres.

Besides the continuation of the preceding work, several other investigations are in progress, assisted by this grant. One of these concerns the effect of pressure on the electrochemical solution tension of metals; another concerns the heat capacity of solutions; and another concerns the atomic weight of sodium.

J. Bishop Tingle, Illinois College, Jacksonville, Ill. Grant No. 40. *For continuing investigations on the derivatives of camphor and allied bodies.* $500.

Abstract of Report.—The work under this grant was not begun till late in the summer. A number of bases have been tested as to their power to undergo condensation with camphoroxalic acid and its ethylic salt. Experiments have also been made to obtain further information as to the possible presence of hydroxyl groups in camphoroxalic acid, with encouraging results.

Engineering.

W. F. Durand, Cornell University, Ithaca, N. Y. Grant No. 64. *For experiments on ship resistance and propulsion.* $4,120.

Abstract of Report.—Professor Durand reports that certain equipment necessary for the conduct of the experiments was completed early in the spring. Experiments in connection with the work on propellers was begun, and all of the work of observation required for the complete determination of the performance of thirty-five

model propellers was finished. To complete the investigation immediately in view, fourteen propellers remain to be experimented with. He feels that the complete experimental determination for thirty-five propellers constitutes a most satisfactory summer's work. This is five-sevenths of the entire field to be covered by this particular investigation. The work of making the detailed reductions and analyses of these observations will presumably occupy most of the winter. But very gratifying progress has been made in the preliminary measurements, speed having been determined from distance and time records in 444 cases and thrust-turning momentum determined by integration from autographic records in 655 cases.

Leonard Waldo, New York city. Grant No. 22. *For study of aluminum bronzes.* $4,500.

Abstract of Report.—Mr. Waldo reports that through the death of his associate, George S. Morison, and the breakdown in health of his chief assistant progress has been slow; he is unable to do more than report progress. He

(*a*) Prepared a bibliography on alloys of aluminum and copper and of other aluminum compounds.

(*b*) Has had in operation six kinds of specially built furnaces, and is building a seventh, to determine the best methods for making large castings and sound wire bars or billets of aluminum bronze.

(*c*) His rolling mill experiments for producing tubes, sheet, wire, and forged bars, from billets cast during the year, are practically complete and are satisfactory.

Notes taken during the process of rolling and cold drawing, relative to temperature, speeds, and cost are awaiting collation and reduction. A complete report will be prepared during the coming year.

Exploration.

Raphael Pumpelly, Newport, R. I. Grant No. 37. *For preliminary examination of the trans-Caspian region.* $6,500.

Abstract of Report.—The reconnaissance covered a region of 1,750 miles in length, with trips from 10 to 300 miles away from the railroad base. Throughout the great part of this area the remains of ancient occupation abound, in the form of large tumuli, village sites, fortresses, and cities.

The structure of the tumuli examined and their contents indicate a very remote beginning and occupation during long periods. The

builders had apparently archaic pottery, no metals, slight knowledge of stone implements, and probably wooden weapons. The people were settled and had the domestic horse, cow, pig, sheep, and goat. Many of these seats of early dwelling seem to have become in time eminences upon which arose fortresses, or to have become the citadels of towns growing up around them. Thus they probably contain the continuous record of the development of the civilizations of the region from a very remote antiquity down to historic times.

The reconnaissance work of Professor Davis, Mr. Huntington, and R. W. Pumpelly has shown the former existence of several glacial epochs, and has made much progress in correlating these with the progress of prehistoric physical events in the building of the plains and the expansions of the former Aralo-Caspian seas. Their observations give reason to hope that further study will correlate these physical events with important phases of human development in connection with Asiatic and European history.

Geophysics.

Frank D. Adams, McGill University, Montreal, Can. Grant No. 4.
For investigating the flow of rocks. $2,500.

Professor Adams has been engaged for some years past in an experimental investigation into the nature of the movements set up during the folding and deformation of the rocks of the earth's crust.

Abstract of Report.—Dr. Adams reports that McGill University has provided for his use in carrying on the investigation on the flow of rocks a large room in the basement of the new chemical building of the University. In this room he has installed the apparatus he formerly had and ordered a third and much more powerful hydraulic press, by which pressure up to 120 tons may be secured and maintained, if necessary, for weeks at a time. Ample provision has been made in the installation of the new hydraulic press, looking to the possibility of the extension of the plant in its adaptation to the most varied experimental uses.

On the completion of the installation Dr. Adams commenced the investigation of high differential pressures on dolomites from Maryland, Massachusetts, and the Province of Quebec. It was found that at ordinary temperatures these dolomites could be made to flow in the same manner as in the case of the pure Carrara marble. He is now carrying on experiments to ascertain the effect of heat upon the flow of dolomite. In order to compare the effects produced at high

pressures with those produced by lower pressures, the higher representing the condition at lower depths in the earth's crust, experiments have been begun on the flow of marble with the 120-ton press.

Dr. Adams is also carrying on a series of investigations into the force required to drive water through Portland oölite, which is the rock he has selected for further experiments on the deformation of limestones when heated, with water passing through them. He has also assembled material to commence the study of granite essexite and diabase, as typical igneous rocks under very high pressures at ordinary temperatures.

C. R. Van Hise, University of Wisconsin, Madison, Wis. Grant No. 71. *For investigating the subject of geophysical research, etc.* $2,500.

In the Year Book for 1902, page 26, an extended report was presented on the subject of geophysics. As the trustees were not prepared to act upon the project, a further study of the problem was made, at the request of the Executive Committee, by Professor Van Hise, who investigated the subject of geophysical research in European institutions and made a report, which is printed in the Accompanying Papers of this report, pages 173–184.

GEOLOGY.

T. C. Chamberlin, University of Chicago, Chicago, Ill. Grant No. 31. *For study of the fundamental principles of geology.* $6,000.

Abstract of Report.—Plans for the consideration of the different phases of the complex subjects of this investigation were arranged with numerous collaborators, and details of this collaboration and the results obtained are given in Professor Chamberlin's report, printed in the Accompanying Papers in this volume, pages 261–270.

Bailey Willis, U. S. Geological Survey, Washington, D. C. Grant No. 72. *For geological exploration in eastern China.* $12,000.

This grant was for the purpose of carrying on a comparative study of the geology of eastern Asia and western North America, by observations in stratigraphy, structure, and physiography in eastern China and Siberia, and by the collection of fossils, particularly with reference to the development of the Cambrian faunas.

He proposed to begin his inquiries in the mountain district in Shantung—the Tai-shan—a geological unit of about 4,000 square miles, where a study could be made of the geology from pre-Cambrian gneisses to the Coal Measures.

Mr. Eliot Blackwelder, an instructor in elementary geology and paleontology in the University of Chicago, accompanied Mr. Willis.

Abstract of Report.—Under date of September 30, 1903, from Tientsin, China, Mr. Willis reports that all preparations are completed, that authority has been received from the Chinese and German governments, and that with his associate, Mr. Blackwelder, he is about to leave for the province of Shantung. From Shantung it is proposed to go to Liautung.

Mr. Willis expects to return to Pekin January 1, 1904, and as soon as may be thereafter to enter upon a trip that will probably continue until the end of June, 1904.

History.

Worthington C. Ford, Washington, D. C. Grant No. 28. *For an examination of the historical archives of Washington.* $2,000.

For the purpose of studying the historical archives of Washington and ascertaining their extent and their characteristics, Mr. Ford prepared a scheme of inquiry which was arranged in two divisions. The first division included a general statement of the contents of each repository of archives, a statement of the place in which it is contained, and the history of the collection; also a statement of the funds available for the maintenance of the collection and of the conditions under which documents are accessible. The second division referred to the preservation of the collections and the arrangements for enlarging them.

Abstract of Report.—The purpose of this grant was to defray the expense of making a general survey of the archives of the government and the preparation of a report which would be helpful to historical investigators. Dr. Claude H. Van Tyne and Mr. Waldo G. Leland began the work in January, 1903, following general suggestions offered by Mr. Ford. They have examined the manuscript material in every branch of the government, and have prepared a statement as to the nature and extent of the administrative records, as well as of the more important collections of historical material. This description is now nearly ready for printing. It will make a

book of 250 or 300 pages of the size of the Year Book. While it does not attempt to describe individual documents, but only classes and collections of documents, it is sure to be helpful to historical scholars seeking material.

PALEONTOLOGY.

E. C. Case, State Normal School, Milwaukee, Wis. Grant No. 46. *For continuation of work on the morphology of Permian reptiles.* $500.

Abstract of Report.—In connection with the preparation of a monograph on the Pelycosauria of the American Permian deposits, Professor Case spent most of the summer in the British Museum and several weeks in the museums of Paris and Berlin in the study of the reptiles of Permian age contained therein. The main line of work resolved itself into a careful comparison of the faunas of the deposits of America, Russia, and South Africa. The most important result was the demonstration that American forms are practically completely different from those of Russia and South Africa, the sole connecting faunas being of the most primitive type and none, so far as known, being common. This emphasizes the peculiarity of the presence of a typical American Pelycosaurian in the deposits of Bohemia. Professor Case also obtained many isolated facts of morphology that will be of material assistance to him in the study of the fauna.

O. P. Hay, American Museum of Natural History. Grant No. 14. *For monographing the fossil Chelonia of North America.* $2,200.

Abstract of Report.—Dr. Hay reports that he has prepared 200 pages of typewritten manuscript, and has had made, under his personal supervision, 210 drawings and 80 photographs of fossil turtles. He finds that there are about 180 species, and that there yet remains much to be done before the monograph will be ready for publication. During the summer he spent two months in the Bridger deposits of Wyoming, collecting fossils, and secured 135 specimens of turtles that will add greatly to our knowledge of Eocene forms.

G. R. Wieland, Yale University, New Haven, Conn. Grant No. 48. *For continuation of his researches on living and fossil cycads.* $1,500.

Abstract of Report.—Dr. Wieland expects to have a memoir ready by the close of the calendar year, dealing with the fossil cycads from

a biological standpoint. He has developed a new method for the study of fossil cycads by perfecting or inventing inverted drills, by means of which he has secured leaves, branches, fruits, flowers, and terminal buds in the form of cylindrical cores cut from the cycad trunks. He has also adopted the novel plan of cementing together again, in their original position, the parts of such cores resulting from the cutting of a series of thin sections, and in this way securing a second series, also complete. By these methods he has cut a dozen fruits, in various stages of growth, from a silicified cycad trunk. He has also cut thin longitudinal and transverse sections of flowers surrounded by leaf bases. It is now possible to make, in the case of cycads, intensive studies of single trunks, such as have never before been made in the case of any fossil plants.

S. W. Williston, University of Chicago, Chicago, Ill. Grant No. 49. *For preparing a monograph on the Plesiosaurian group.* $800.

Abstract of Report.—Professor Williston reports that he investigated the type material of Plesiosaurs at Colorado College, University of Kansas Museum, the American Museum of Natural History in New York, the Museum of the Academy of Natural Sciences, Philadelphia, and the National Museum, Washington. Important material has been sent him from these and other sources, upon which he is at present engaged. He hopes to complete his study during the year 1904.

Physics.

Henry Crew, Evanston, Ill. Grant No. 10. *For study of certain arc spectra.* $1,000.

Abstract of Report.—Professor Crew reports that after the building of certain apparatus, which required several months, he began the experimental part of his work. He found unexpected difficulties in working with magnesium and zinc, the two metals in which he hoped to find the order of appearance of the lines of the spark spectra.

His second problem was to complete the maps of the spectra of cadmium and aluminum. The map of the cadmium arc has been completed; that of aluminum nearly so.

The difficulty of obtaining an oscillograph has delayed the beginning of work on the third problem, the determination of the E. M. F. curve with the "rotating metallic arc."

A. A. Michelson, University of Chicago, Ill. Grant No. 47. *For aid in ruling diffraction gratings.* $1,500.

Abstract of Report.—Professor Michelson encountered many serious difficulties in the ruling engines for diffraction gratings, most of which he now believes are overcome. The work is now being pushed vigorously, and he hopes before another year to make a favorable report on the results obtained.

Harold Pender, Johns Hopkins University, Baltimore, Md. Grant No. 18. *For experiments on the magnetic effect of electrical convection.* $750.

Abstract of Report.—The object of Dr. Pender's grant was to perform in Paris, in conjunction with Mons. B. Cremieu, experiments on the magnetic effect of electrical convection and to confer with M. Poincaré concerning the same. Dr. Pender met with great success in clearing up a controverted question as to the presence of a magnetic field about a bare metallic surface when charged and set in motion, which field is in all probability due to what is usually termed a convection current of electricity.

R. W. Wood, Johns Hopkins University, Baltimore, Md. Grant No. 25. *For research, chiefly on the theory of light.* $1,000.

Abstract of Report.—Professor Wood reports that one half of the grant has been expended for the salary of an assistant, and that the balance he plans to expend for apparatus. Through the aid given he was able to accomplish much more experimental work than he otherwise could have done. During the year he obtained results which were published in seven papers, all of which pertain to researches connected with the theory of light.

A considerable amount of work was also done on an investigation on the dispersion of sodium vapor; this has not yet been published.

PHYSIOLOGY.

W. O. Atwater, Wesleyan University, Middletown, Conn. Grant No. 5. *For experiments in nutrition.* $5,000.

Abstract of Report.—The purpose of this grant was to promote researches involving the direct determination of the amount of oxygen consumed by man for sustaining the bodily functions. The grant has been expended chiefly for the services of experts and assistants,

for devising and constructing or purchasing apparatus, for developing methods for the determination of oxygen, and for efficiency tests and experiments with men in the apparatus.

Several tests of the efficiency of the apparatus and method of manipulation were made. The feasibility of the use of the apparatus for the experiments with men has also been tested by three experiments with different subjects, with satisfactory results. Attention is now being devoted to alterations and improvements in the apparatus and to modifications of methods; efficiency tests and experiments with men are also in progress.

Arthur Gamgee, Montreux, Switzerland. Grant No. 62. *For preparing report on the physiology of nutrition.* $6,500.

Abstract of Report.—Dr. Gamgee began and has carried on a study of the extensive literature on this subject, which had to be mastered for the purpose of the inquiry on which he was engaged. He began by inspecting European laboratories and by visiting scientific men in Europe. He also visited Professor Atwater, at Middletown, Conn., and acquainted himself with the work now in progress there. He also visited other Americans. It is probable that his complete report will be transmitted in May, 1904.

Psychology.

G. Stanley Hall, Clark University, Worcester, Mass. Grant No. 61. *For certain investigations on the anthropology of childhood.* $2,000.

Abstract of Report.—The result of Dr. Hall's work in connection with this grant is best indicated by the titles of the papers he has published, giving the results obtained during the year. These are:

1. Reaction to light and darkness.
2. Children's ideas of fire, heat, frost, and cold.
3. Curiosity and interest.
4. Showing off and bashfulness as phases of self consciousness.
5. Marriage and fecundity of college men and women.

E. W. Scripture, Yale University, New Haven, Conn. Grant No. 21. *For researches in experimental phonetics.* $1,600.

Report.—Professor Scripture's report is printed in the Accompanying Papers in this volume, pp. 243–259.

ZOÖLOGY.

H. E. Crampton, Columbia University, New York. Grant No. 9. *For determining the laws of variation and inheritance of certain lepidoptera.* $250.

Abstract of Report.—In order to obtain data for the problems of variation, their relation to selection, and for the study of correlation, Dr. Crampton investigated the following material:

(*a*) Eight hundred and forty-eight cocoons of *Philosamiacynthia.*

(*b*) Fourteen hundred and ten cocoons of *Samia cecropia.*

(*c*) Four hundred cocoons of *Callosamia angulifera*, etc.

(*d*) Seventy-five cocoons (preliminary) of *Attacus orizaba.*

(*e*) One family, *Hypercheiria io.*

The data secured furnish material for examination into variation and selection by comparing—

(*a*) Metamorphosing and non-metamorphosing.

(*b*) The perfect and imperfect survivors.

(*c*) The mating and non-mating moths.

Dr. Crampton thinks that certain general conclusions are justified from the facts already determined. Surviving individuals are less variable than those which succumb; mating individuals are less variable than those which fail to mate; and the index of correlation of the pupal characters is higher for the selected individuals in both cases. In a word, selection proceeds upon a basis of deviations from type and upon a correlative basis.

J. E. Duerden, Chapel Hill, N. C. Grant No. 12. *For investigation of recent and fossil corals.* $1,000.

Abstract of Report.—With a view to obtaining suitable material for continuing his researches on fossil corals, Dr. Duerden has lately visited the principal museums and geological surveys in Great Britain, where Paleozoic corals are most abundant. These museums, and also the Smithsonian Institution, have placed at his disposal numerous specimens. Other material has been purchased. These collections will be studied during the present winter, with the hope of showing the relationship of fossil to recent corals.

Dr. Duerden has deposited with the Carnegie Institution, with a view to its publication, the manuscript and drawings of a memoir entitled "The coral *Siderastræa radians* and its post-larval development." This work is illustrated by fifteen plates and numerous text

figures and gives an account of the morphology of a coral and its growth for a period of four months. It carries the development of the coral much farther than any previous work and contains many fundamental results in madreporarian morphology.

C. H. Eigenmann, Indiana University, Bloomington, Ind. Grant No. 68. *For investigating the blind fishes of Cuba.* $1,000.

Abstract of Report.—Dr. Eigenmann did not begin his work under the Carnegie grant until October. He expects to spend from four to six months in Cuba, during the entire breeding season, and to make general collections in the caves and streams. He will also make an effort to secure the blind fishes from the island of Jamaica. He has made arrangements with the Cuban government to coöperate with him, as far as practicable, in giving him facilities for carrying forward his investigation.

L. O. Howard, Department of Agriculture, Washington, D. C. Grant No. 38. *For preparing manuscript and illustrations for a monograph on American mosquitoes.* $2,000.

Abstract of Report.—Dr. Howard began his work by making arrangements to secure observers at points in the United States, Central America, and the West Indies sufficiently different in their faunistic characteristics to promise comparatively little duplication. He also published an announcement of the proposed monograph for the purpose of attracting volunteer observers and contributors; and, through correspondence, a great deal has been done in that direction, both in the West Indies and the United States. He also utilized the services of a number of the members of his force in the Department of Agriculture in making collections and observations.

He reports that the results as a whole have been surprising to him. A number of new species of mosquitoes have been discovered and one new genus, and much important specific information regarding the geographic distribution of the different species has been gained. This information has been of special interest and value regarding the yellow fever mosquito (*Stegomya fasciata*) and the different species of the malaria bearing mosquitoes of the genus Anopheles. A new species of this genus was found in the immediate vicinity of Washington. Great advance has been made in following out the life histories of the different species and genera; this has been done for nearly one hundred species.

All the collections and specimens have not yet been received by Dr. Howard, but every observer will send a series of specimens of adults, eggs, larvæ, and pupæ, together with cast larval skins of all species observed. These have been and will be accompanied by full notes of habits, etc., together with drawings of structural peculiarities.

H. S. Jennings, University of Michigan, Ann Arbor, Mich. Grant No. 15. *For experiments on the behavior of lower animals.* $250.

Abstract of Report.—Dr. Jennings, who is a research assistant of the Carnegie Institution, is now at the Marine Biological Laboratory at Naples, carrying forward investigations on the reactions and behavior of very low organisms, such as Amœba and other Rhizopoda. He expects to have a general work in regard to the behavior of the lowest organisms ready for publication during the year. He has submitted to the Institution for publication a paper entitled "Reactions to heat, light, and other stimuli in the ciliate infusoria and in Rotifera, with considerations on the theories of animal behavior."

C. E. McClung, Kansas University, Lawrence, Kans. Grant No. 16. *To making a comparative study of the spermatogenesis of insects and other classes of arthropods, and if possible to determine the specific functions of the different chromosomes.* $500.

Abstract of Report.—Professor McClung reports that owing to the fact that his own work and that of others show the main features of insect spermatogenesis, he determined to make use of the grant for the prosecution of other more difficult and expensive studies. He commenced by purchasing some literature to which he did not have access, and began the search for an object upon which he might prosecute his investigations. There appeared to be two ways to get at the problem—to study the germ cells of hybrids or to experiment upon fertilized eggs in the early cleavage stages. He decided to adopt the first mentioned plan for the beginning of the work. With this object in view, he spent the summer at the Woods Hole Marine Biological Laboratory, but did not succeed in obtaining satisfactory forms of hybrids. He feels certain, however, that if the proper animals are secured the true function of the chromosomes may be settled as definitely as any other fact relating to cell structure.

E. B. Wilson, Columbia University, New York. Grant No. 36. *For investigations in experimental embryology, etc., in Naples.* $1,000.

Abstract of Report.—Dr. Wilson utilized this grant to defray the expenses of a visit to the Naples Zoölogical Station, extending from February to July, during which time he was actively engaged on studies in experimental embryology. His first purpose was to search for available material for the experimental analysis of the early developmental stages in mollusks and annelids, which possess high theoretical interest in their bearings on the general problems of differentiation. He reports a large measure of success in this direction. He found two excellent objects for his research, and made as exhaustive an analysis of them as the time would permit. He demonstrated conclusively the mosaic character of the development in the molluscan egg, and obtained striking evidence of the self differentiation and specification of embryonic cells. This result is interesting from its bearing on the problem of differentiation and also, perhaps, in even a greater degree, through the firm basis which it gives for the general method and point of view in studies of cellular embryology.

A second general division of his work included the experimental study of prelocalization in the unsegmented egg, which yielded results of no less interest than the cleavage stages. Of these the most important relate to the embryonic basis of correlation and to the relation between quantitative and qualitative prelocalization in the germ.

Dr. Wilson adds a general comment on the nature of this work to the effect that its principal significance lies in its connection with recent studies of the cellular basis of inheritance and development, taken in connection with experimental studies of heredity such as those that have grown out of the rediscovery of the Mendelian law. He is fully persuaded that there is now a very good prospect of making an essential advance toward an understanding of the actual mechanism of hereditary transmission, and expresses the hope that the studies in this direction may receive their due share of support.

H. V. Wilson, University of North Carolina, Chapel Hill. Grant No. 33. *For morphology and classification of deep sea sponges.* $1,000.

Abstract of Report.—In order to complete his investigation of the deep sea sponges of the Pacific ocean, Professor Wilson visited the

museums of London, Paris, Leiden, and Berlin to make a direct examination of the types stored therein. He returned to America in August, and is at present engaged upon the text of his report.

Marine Biological Laboratory, Woods Hole, Mass.; J. Blakely Hoar, treasurer. Grant No. 35. *For maintenance of 20 tables*, $10,000.

Abstract of Report.—This appropriation was made for the purpose of aiding the laboratory by paying for the maintenance of twenty research tables. The persons assigned to the tables were selected by the Carnegie Institution.

The following investigators occupied the Carnegie tables during the season of 1903:

Names.	Dates.
1. Prof. M. A. Bigelow . . . . Columbia University, N. Y.	May 30 to Aug. 8.
2. Dr. R. M. Strong . . . . University of Chicago, Ill.	June 16 to Sept. 18.
3. Prof. C. E. McClung . . . . University of Kansas, Lawrence.	July 6 to Aug. 24.
4. Prof. George Lefevre . . . . University of Missouri, Columbia.	July 28 to Aug. 19.
5. Prof. Wm. E. Kellicott . . . . Barnard College, N. Y.	June to August.
6. Prof. Arthur W. Greeley . . . . Washington University, St. Louis.	June 25 to Aug. 15.
7. Mr. C. J. Brues . . . . Columbia University, N. Y.	June 30 to Sept. 1.
8. Mr. Fred. E. Pomeroy . . . . Bates College, Lewiston, Me.	June to August.
9. Mr. J. W. Scott . . . . University of Chicago, Ill.	June 22 to Aug. 24.
10. Dr. H. G. Spaulding . . . . College of the City of New York.	June to August.
11. Dr. Leo Loeb . . . . McGill University, Montreal, Canada.	July 11 to Sept. 5.
12. Dr. Henry Kraemer . . . . Philadelphia, Pa.	July 13 to Aug. 10.
13. Mr. Grant Smith . . . . Harvard University, Cambridge, Mass.	July to August.
14. Prof. Joseph Guthrie . . . . Iowa State College, Ames, Iowa.	July to August.
15. Miss A. B. Townsend . . . . Cornell University, Ithaca, N. Y.	July to August.
16. Mr. M. A. Chrysler . . . . University of Chicago, Ill.	July to August.

17. Mr. Gustav Ruediger . . . . July 7 to Aug. 17.
    Chicago, Ill.
18. Miss Helen Dean King . . . . June 10 to July 13.
    Bryn Mawr University, Pa.
19. Mr. James A. Nelson . . . . July to August.
    University of Pennsylvania.
20. Prof. Christian P. Lommen . . . . . . July to August.
    University of South Dakota.

The Director of the Laboratory, Dr. C. L. Whitman, reports that the entire number of investigators at the laboratory during the season was 130, of whom 54 were students and 76 original investigators.

He further states that every worker at the laboratory shares the general advantage secured by the Carnegie Institution grant; that most of the occupants of the Carnegie tables were investigators of established reputation, a few of them Fellows from different universities engaged in their first original work; that it is not expected that the work undertaken will come to publication immediately, as in most cases it will necessarily extend over two or three years; that it is anticipated that the Carnegie support will not encourage hasty and fragmentary production, but will secure thorough work and permanent results.

Marine Biological Station, Naples, Italy. Grant No. 55. *For maintenance of two tables.* $1,000.

Abstract of Report.—One of the tables at this station was occupied for three months during the spring by Dr. E. B. Wilson, of Columbia University, and the other by Professor H. S. Jennings, of the University of Michigan.

The remainder of the year the tables were open to whomever the director of the laboratory might wish to assign to them.

The arrangement with the laboratory was that the tables were intended for the use of persons engaged in original biological researches, and carried with them the right to be furnished with the ordinary material and supplies of the laboratory.

Student Research Work in Washington, $10,000.

A special committee was appointed to consider the question of making provision for training in Washington students who desire to avail themselves of the various openings that may be offered to them. The Executive Committee, after full discussion, decided to place the report of the special committee on file, without action.

Research Assistants.

In pursuance of the policy approved by the Trustees at their meeting in November, 1902, the sum of $25,000 was set aside by the Executive Committee for the purpose of assisting a certain number of young investigators who have shown exceptional ability and desire to pursue special lines of inquiry, under the oversight of qualified guides, more or less authoritative, according to the circumstances of each case.

Announcement of this plan was made by a printed circular, which was published in the winter of 1902–1903, and addressed to the heads of universities, colleges, laboratories, and other scientific institutions. It reads as follows :

"It is the purpose of the Carnegie Institution of Washington, among other plans, to encourage exceptional talent by appointing a certain number of Research Assistants.

"These positions will not be those commonly known as 'Fellowships' or 'Scholarships'; nor is the object of this provision to contribute to the payment of mechanical helpers or of assistants in the work of instruction. It is rather to discover and develop, under competent scrutiny and under favorable conditions, such persons as have unusual ability. It is not intended to provide means by which a student may complete his courses of study, nor to give assistance in the preparation of dissertations for academic degrees. Work of a more advanced and special character is expected of all who receive appointment.

"The annual emolument will vary according to circumstances. As a rule, it will not exceed $1,000 per annum. No limitations are prescribed as to age, sex, nationality, graduation, or residence. Appointments will at first be made for one year, but may be continued.

"It is desirable that a person thus appointed should work under the supervision of an investigator who is known to the authorities of the Carnegie Institution to be engaged in an important field of scientific research, and in a place where there is easy access to libraries and apparatus, but there may be exceptions to this.

"Applications for appointments may be presented by the head of or by a professor in an institution of learning or by the candidate. They should be accompanied by a statement of the qualifications of the candidate, of the research work he has done, and of that which he desires to follow, and of the time for which an allowance is desired. If he has already printed or written anything of interest, a copy of this should be enclosed with the application.

"Communications upon this subject should be distinctly marked on the outside envelope, and on the inside, Research Assistant, and should be addressed to the Carnegie Institution of Washington."

In response to this announcement 127 applications were received. These were distributed according to the subjects of investigation and referred to the confidential advisers, whose written opinions were laid before the Executive Committee with accompanying papers. The persons below named were then selected :

J. H. Bair, Columbia University, New York, N. Y.	Grant No. 73.
J. W. Baird, Cornell University, Ithaca, N. Y.	Grant No. 74.
A. J. Carlson, Stanford University, California	Grant No. 75.
C. D. Child, Colgate University, Hamilton, N. Y.	Grant No. 76.
Arthur B. Coble, Lykens, Pa.	Grant No. 65.
W. W. Coblentz, Cornell University, Ithaca, N. Y.	Grant No. 77.
Lee H. Cone, University of Michigan, Ann Arbor, Mich.	Grant No. 78.
Elias Elvove, Lexington, Ky.	Grant No. 79.
Shephard I. Franz, Hanover, N. H.	Grant No. 80.
L. E. Griffin, Missouri Valley College, Marshall, Mo.	Grant No. 81.
Ellsworth Huntington, Milton, Mass.	Grant No. 82.
Herbert S. Jennings, Ann Arbor, Mich.	Grant No. 83.
George D. Louderback, Reno, Nev.	Grant No. 66.
Albert P. Morse, Wellesley, Mass.	Grant No. 84.
C. P. Neill, Catholic University, Washington, D. C.	Grant No. 85.
Hideyo Noguchi, University of Pennsylvania, Philadelphia	Grant No. 29.
James B. Overton, Jacksonville, Ill.	Grant No. 86.
H. F. Perkins, University of Vermont, Burlington, Vt.	Grant No. 87.
H. N. Russell, Kings College, Cambridge, England	Grant No. 88.
George W. Scott, University of Pennsylvania, Philadelphia	Grant No. 60.
R. M. Strong, Haverford, Pa.	Grant No. 89.
H. G. Timberlake, University of Wisconsin, Madison	Grant No —.
J. B. Whitehead, Jr., Johns Hopkins University, Baltimore	Grant No. 59.
E. J. Wilczynski, Berkeley, Cal.	Grant No. 58.
F. S. Wrinch, Princeton, N. J.	Grant No. 91.

One of the persons thus selected, Mr. H. G. Timberlake, died in July, 1903, and one of them, Mr. C. D. Child, did not accept the appointment on account of a change in his plans. From all the others satisfactory reports of progress have been received, which again have been referred to specialists for their scrutiny and comment.

It is the purpose of the Executive Committee, upon the final examination of the reports of the work of these Research Assistants, to select those whose work justifies the opinion that their capacity warrants further grants by the Institution to enable them to proceed with their investigations, and to continue the grants to the persons so selected and discontinue them as to the remainder of the list.

The specific subjects to which these twenty-five investigators proposed to direct their attention were distributed among the following

branches of science: Astronomy, 1; Botany, 2; Chemistry, 2; Economics, 1; Geology, 2; History, 1; Mathematics, 2; Physics, 3; Physiology, 2; Psychology, 3; Zoölogy, 6.

The geographical distribution of these students cannot be very accurately stated, as their early homes are not known to the Carnegie Institution; but indications may be derived from a list of the colleges in which the preliminary academic training was received:

Augustana College	Carlson.
Beloit College	Huntington.
California, University of	Louderback, Wilczynski.
Columbia University	Franz.
Fukushima, Japan, Provincial High School	Noguchi.
Georgetown University	Neill.
Hamline College	Griffin.
Johns Hopkins University	Whitehead.
Kentucky State College	Elvove.
Lake Forest College	Timberlake.
Michigan, University of	Bair, Jennings, Overton.
Oberlin College	Strong.
Pennsylvania College	Coble.
Pomona College	Cone.
Princeton University	Russell.
Stanford University	Scott.
Toronto, University of	Baird, Wrinch.
Vermont, University of	Perkins.

It is also interesting to mention the places where their post-graduate studies were pursued:

Augustana College	Carlson.
California, University of	Carlson, Louderback, Wilczynski.
Cambridge, University of (England)	Child, Russell.
Chicago, University of	Overton, Scott.
Columbia University	Bair, Franz, Scott.
Cornell University	Baird, Child, Coblentz, Scott.
Hamline College	Griffin.
Harvard University	Huntington, Jennings, Strong.
Johns Hopkins University	Coble, Griffin, Neill, Perkins, Whitehead.
Kentucky State College	Elvove.
Leipzig, University of (Germany)	Franz.
Michigan, University of	Bair, Cone, Timberlake.
Pennsylvania, University of	Noguchi, Scott.
Princeton University	Russell.
Stanford University	Carlson.
Wellesley College	Morse.
Wisconsin, University of	Baird, Timberlake.
Wurtzburg University	Wrinch.

PUBLICATIONS AUTHORIZED.

The publication of eleven scientific papers has been authorized.

1. The collected mathematical works of the astronomer George William Hill. It is estimated that these works will make four quarto volumes. About half of volume I is printed.

2. Desert Botanical Laboratory of the Carnegie Institution, by F. V. Coville and D. T. MacDougal. This is an octavo containing 58 pages, 29 plates, and 4 text figures. Published.

3. New method for determining compressibility, by T. W. Richards and W. N. Stull. This is an octavo of 45 pages and 5 text figures. Published.

4. Waterlilies—a monograph of the genus *Nymphæa*, by H. S. Conard. This is to be a quarto containing 28 plates (12 being colored) and about 80 text figures. The text figures are made, and contracts have been awarded for plates and text.

5. Fecundation in plants, by D. M. Mottier. Manuscript received, and the drawings for text figures, about 300, have been made.

6. On the behavior of lower organisms, by H. S. Jennings. Manuscript received and accepted for publication.

7. The coral Siderastrea, by J. E. Duerden. Manuscript received and accepted for publication.

8. Catalogue of double stars, by S. W. Burnham. Manuscript ready for the press.

9. Chimera—a memoir on the embryology of primitive fishes, by Bashford Dean. Manuscript not received.

10. Bibliographic index of North American fungi, by W. G. Farlow. Will make five octavo volumes.

11. Results of investigations of poison of serpents, by Drs. Simon Flexner and Hideyo Noguchi. Manuscript not received.

RECOMMENDATIONS OF EXECUTIVE COMMITTEE.

The Executive Committee submitted a series of recommendations for the work of the fiscal year 1903–1904, which were considered and acted upon by the Board of Trustees.

The results of this action will be reported to the Trustees at the meeting in December, 1904, and be printed in the Year Book.

Applications for Grants.

The Committee has declined to make any grants in medicine or for preparing systematic treatises or essays in logic and philosophy.

All applications, from the beginning of the Institution to October 31, 1903, are summarized in the following table:

List of Applications Received from Beginning to November, 1903.

Subject.	Applications.			Amount asked for.
	Not stating amount desired.	Stating amount desired.	Total.	
Agriculture	3	1	4	$5,000
Anthropology	26	18	44	90,083
Archeology	11	5	16	17,700
Art	10	....	10	....
Astronomy	21	37	58	567,750
Bibliography	15	12	27	82,250
Biology	14	1	15	100,000
Botany	28	32	60	138,300
Chemistry	37	52	89	90,500
Economics	38	8	46	72,500
Education	20	1	21	500
Engineering	20	5	25	24,040
Exploration	2	3	5	110,000
Fellowship	39	2	41	1,700
Foreign applications	7	8	15	17,000
Geography	1	2	3	1,500
Geophysics	3	9	12	33,250
Geology	21	16	37	145,800
History	30	9	39	101,400
Inventions	21	2	23	2,100
Literature	10	....	10	....
Mathematics	11	9	20	13,525
Medicine	35	11	46	16,325
Meteorology	2	6	8	32,750
Miscellaneous	25	7	32	68,200
Paleontology	5	5	10	11,900
Philology	12	1	13	750
Psychology	22	15	37	77,600
Physics	32	26	58	37,350
Physiology	23	20	43	30,975
Publication	37	18	55	90,250
Religion	9	2	11	37,000
Zoölogy	46	63	109	182,400
Total	636	406	1,042	$2,200,398

Grants Recommended by Advisory Committees.

In addition, the Advisory Committees have submitted a number of recommendations not included in the foregoing table. These are printed on pages xxxiv–xxxv of the confidential report to the Trustees, issued November 11, 1902, and that for the southern and solar observatories in the present report:

Physics, per annum	$250,000
Geophysics, per annum	150,000
Psychology, per annum	45,000
Physiology, per annum	50,000
Southern Observatory, twelve years ($820,000), first year	80,000
Solar Observatory, twelve to fourteen years ($1,280,000), first year	150,000
History, per annum	17,500
Botany, per annum	24,000
Exploration, per annum	120,000
Geology, three years, per annum	25,000
Total	$911,500
Adding this to the total amount in above summary	2,200,398
Gives a total of	$3,111,898

The above total would have been still larger if all the grants had been made as requested. Frequently grants are requested for one year which, if made, would involve a number of subsequent grants before the completion of the work.

This is not intended as a close analysis of the amount of money desired. It merely shows the impossibility of making the present income of the Carnegie Institution provide for more than a small part of the grants requested.

Substantially all these applications have been carefully examined and considered. Many of the more important are explained in the first Year Book. All are set forth and explained in the papers on file and ready to be produced for the consideration of the Trustees.

Most of these applications have been considered unfavorably by the Committee as being for less desirable purposes for expenditure from the income of the trust than those for which grants have been made. Some, however, have seemed only less important than the matters favorably reported upon, and these should, the Committee thinks, be regarded as subjects of future consideration whenever available funds shall permit.

MEMORIALS.

Abram S. Hewitt.

Abram Stevens Hewitt, one of the confidential advisers of Mr. Carnegie respecting the foundation of this Institution, and by election the original Chairman of the Board, died at his home, in New York, January 18, 1903, aged 81 years. By his death the Trustees have met an irreparable loss. The Cooper Union, with which he was intimately associated during all its history, has published a summary of his life so excellent in all respects that the paragraphs of general interest will be here reproduced.

Abram Stevens Hewitt was born in Haverstraw, N. Y., July 31, 1822. His father, John Hewitt, an English mechanic, came to this country in 1790, to assist in erecting the first steam engine in America, and remained to share the fortunes of the young Republic. His mother belonged to a Hugnenot family (the Garnier, now the well known Gurnee family) resident in the State of New York.

At the time of his birth, his father, after acquiring a considerable fortune as a cabinet maker and merchant in New York City, and losing all by a disastrous conflagration, had become a farmer upon the Garnier estate; and in a log-cabin on this tract he was born.

Working upon the farm in the summer and attending a New York public school in the winter, he won at the end of his common-school course, as the result of a severe competitive examination, one of the two free scholarships then offered annually by Columbia College, and was thus enabled to go through a college course, supporting himself by extra labor as a private tutor, so that he could afterwards say with just pride, "Not one dollar of burden did my education impose upon my parents, who anxious as they might be to give me an education, were too poor to do so."

In college, as in school, he held from first to last the position of head of his class, and, after graduation, was for some time assistant professor in mathematics. During this period he began the study of law; and in 1845 he was admitted to the bar, after an examination of exceptional severity, which the majority of the candidates failed to pass.

Meanwhile, however, he had made a brief trip to Europe, together with his friend Edward Cooper, and, shipwrecked on the return voyage, had been, with his companion, rescued after drifting for a day in a frail boat upon the Atlantic. His subsequent statement as to the impression produced by this experience is given in his own words, because it sounds the keynote of his life.

"I landed at New York in mid-winter, in a borrowed suit of sailor's clothing, and I had three silver dollars in my pocket, my entire worldly wealth.

"I was then twenty-two years old; and that accident was the turning-point of my life. It taught me, for the first time, that I could stand in the face of death without fear and without flinching. It taught me another thing—that my life, which had been thus miraculously rescued, belonged not to me; and from that hour I gave it to the work which from that time has been in my thoughts—the welfare of my fellow-citizens."

His companionship in travel and peril with Edward Cooper led to relations with Peter Cooper, in consequence of which he abandoned his intention to practice law, and formed with his friend the firm of Cooper & Hewitt, which assumed the iron branch of Peter Cooper's business, and the long, enterprising, and honorable career of which is part of the commercial history and progress of the United States.

The key to Mr. Hewitt's life is given by his words above quoted. He devoted himself to "the welfare of his *fellow-citizens.*" This is not to be construed as limiting his philanthropic sympathy to American citizens alone. Many generous acts prove that he drew no such rigid line. But, as illustrated by his life, it does show plainly that his cherished aim was patriotic as well as philanthropic. With Peter Cooper, he believed in the Republic, and in its free institutions as furnishing the necessary and sufficient atmosphere for individual well-being and progress. And he believed that knowledge, or rather the opportunity to acquire knowledge, was the one thing that could be bestowed gratuitously by the State without pauperizing the recipient. That being given, the maintenance of freedom, justice, and order would complete all that government could wisely undertake from the standpoint of internal administration.

The period of his childhood was the first age of our national history—an age of ardent patriotism and undaunted enterprise and adventure. And the inspiration of this period unquestionably continued to be with him a motive power. But his manhood was cast in a new and different age—that of the material conquest of a continent, the defense of national unity, and the development of industry and commerce. The questions thus encountered called for not only stanch patriotism, but also a knowledge of law and practical business; and in these respects Abram S. Hewitt was thoroughly equipped for illustrious service.

During the war of 1861–5, though always politically a Democrat, he gave the government a hearty support. In fact, the iron works of Cooper & Hewitt were for four years largely given up to the manufacture of munitions of war, without profit to the firm, but (as the War Department has repeatedly acknowledged) to the great and in some instances decisive advantage of the Union cause.

In 1867 Mr. Hewitt was one of the U. S. Commissioners to the Paris Exposition, and his report on the manufacture of iron and steel, as illustrated at that exposition, produced a profound impression at home and (through translations in several languages) abroad. Years before, his firm had made experiments with the Bessemer process; and after 1867, it undertook the introduction into America of the "open-hearth" process. Still later, it was concerned in the adoption of the "basic-lining" principle, applicable to both these methods; and thus it may fairly be said to have had a vital connection with the methods which now cover the manufacture of nearly all the steel (except the so-called crucible-steel) of this country and the world. For his services in connection with these great improvements, the Iron and Steel Institute awarded to Mr. Hewitt, in 1890, the "Bessemer Gold Medal." Meanwhile, the iron-works of Cooper & Hewitt in various localities exhibited the results of the latest scientific practice, and stood, at times, at the head of American practice—a position which no single concern can long continue to hold.

In 1876, and again in 1890, Mr. Hewitt was elected President of the American Institute of Mining Engineers; and his presidential addresses during both terms

attracted wide attention and have become classics in the literature of the subjects with which they deal.

His important and influential Congressional career began in 1874, when he was elected to the House of Representatives. Reëlected in 1876, he declined in 1878 to be a candidate; but in 1880 he was again returned, and held his seat by successive reëlections until, in 1886, he was chosen Mayor of New York.

His work in Congress was more profoundly important, perhaps, than has ever been recognized. While he often differed with the leaders of his party, he was always relied upon by them, and even by his party opponents, for wise advice and intelligent information. In the reform of the consular service, the resumption and maintenance of specie payments, the defeat of the "free" coinage of silver, the establishment of the National Geological Survey, and the peaceful solution of the Presidential electoral contest of 1876, his attitude and arguments may be said to have been decisive factors. To the various tariff debates, he contributed a business knowledge, sometimes distasteful, but always useful, to theoretic partisans. The successive Morrison, Mills, McKinley, and Dingley tariff bills embodied, apart from their schemes of duties, administrative details which he had furnished. He secured also the adoption of the extensive plan for the improvement of New York Harbor, which since has been steadily prosecuted.

In 1886 he was elected Mayor of New York City. To the duties of that office he devoted with intense assiduity the powers which had been developed and matured by a lifetime of varied experience; and his administration, though hampered by local partisan conditions, left behind it many beneficent effects and many suggestions which have since borne fruit. Of these, one of the most permanently important is the plan of municipal rapid transit which he devised, and which, though at first ignored by the Board of Aldermen and smothered in the legislature at Albany, was at last forced to adoption by the public sentiment, and is now in process of execution. Its essential feature is that the work, constructed with the money, and actually the property, of the city, shall be leased to a responsible corporation at a rental covering both the interest on its cost and a sinking fund which will repay in fifty years the principal; so that, at the end of that period, the whole rapid-transit system will be the property of the city, free of all cost, even of interest *ad interim*, and of all obligations to any private or corporate interest. A fairer or more ingenious method of dealing with a great public franchise, without imposing burdens upon the present generation for the benefit of the next, it would be difficult to devise.

In April, 1900, the Chamber of Commerce of the State of New York elected Mr. Hewitt to honorary membership, in recognition of "his long and valuable services to the city, State, and nation, and with special regard to his initiation in this body, of the rapid-transit plan, under which the contract was awarded, and the work is now proceeding." At the same meeting, a gold medal was ordered to be prepared and presented to Mr. Hewitt, for his services in the cause of rapid-transit under municipal ownership.

For more than forty years from the beginning of the Cooper Union until his death he was one of the trustees and the Secretary of the Board. During the whole period he was practically the General Superintendent of the Institution, and with great capacity and untiring devotion supervised its work and affairs and devised and carried into effect plans for the extension of its usefulness. His

personal contributions in money have been over two hundred thousand dollars, and no doubt it has been largely due to his personality and to the work he has done for the Cooper Union that generous donors not related to Mr. Cooper have made it the recipient of their munificent contributions to its endowment fund.

William Earl Dodge.

William E. Dodge, of New York, was the first person elected by the Trustees of the Carnegie Institution to membership in the Board. Their estimate of his character has been given upon a preceding page, and now a brief summary of the events of his life will be added.

Our colleague bore the full name of his father, William E. Dodge, once a member of Congress, eminent for his virtues as a merchant, a philanthropist, and a public-spirited citizen. The son was born in New York, February 15, 1832, and throughout his long life was connected, in one capacity or another, with most of the commercial, religious, and beneficent institutions of the metropolis. In many of them he had an hereditary interest. For fifty-three years he was connected with the house of Phelps, Dodge, and Co., of which at his death he was the senior member. As his years advanced, his influence increased, and his counsel was sought in the promotion of many important enterprises. Originally engaged in the importation of metals, he subsequently took part in various mining and manufacturing industries and in railroad transportation. He became a Trustee of the Metropolitan Museum of Natural History, and of the Botanical and Zoölogical Gardens of New York. The Columbia School of Mines honors him among its earliest friends. He gave liberally to many colleges, and among them he was particularly interested in the Teachers' College of New York. Of the Young Men's Christian Association in New York he was one of the earliest members, and of the Evangelical Alliance a lifelong supporter. In the promotion of Southern education he was a wise counselor, and the advocacy of international arbitration lay very near his heart. In the lapse of time his services in the United States Sanitary Commission may have passed from remembrance, but they were important, and recently his influence in the monetary conference at Indianapolis was most useful. His good deeds are well enumerated in the following words by Dr. L. T. Chamberlain, who knew him well:

> His executive force was in keeping with his rare perception. In passing from principle to practice, he lost no whit of his preëminence. He knew men. He understood means and measures. Though not of stalwart physique, his capacity for toil was prodigious. Neither did difficulties appal him, nor delays

discourage him. When he had resolved on the desired end, no course was too laborious, and scarce any process too costly, for his self-denying adoption. Witness his organizing work in the Sanitary Commission of the Civil War; his guidance of the great plan of Young Men's Christian Associations; his part in the development of the Students' Volunteer Movement; his furtherance of the cause of Arbitration between his country and Great Britain; his promoting of the Federation of Churches and Christian Organizations in this city; his service in connection with the Peabody and Slater Funds; his quiet, effective advocacy of "sound money"; his labors as Chairman of the Committee of One Hundred on India Famine Relief; his unmeasured and priceless devotion to the Metropolitan Museum of Art, the Museum of Natural History, and the Botanical Garden; his practical interest in all matters of civic and social betterment; his influential place in nearly all the great movements of the Chamber of Commerce, in his day; his carrying forward of the notable Conferences held by his Alliance; to say nothing of his successful control of his own varied and important business undertakings.

And the same writer adds this appreciation:

Though not scholastic after the manner of the schools, he was truly widely cultured. Notwithstanding his unremitting preoccupations as a man of affairs, his reading was extensive, and what he read became his own through comprehension and remembrance. Those who knew him best most wondered at his real acquaintance with the noblest books Nor was his acquaintance with noble men and women less ample. Here, and abroad, and in every walk in life, he had friends whom it was well worth while to know. His home was hospitable to the wise and good from far and near. With the worthiest he had exalted friendship. He was a good writer. He was a good critic of writing. He was an accomplished speaker.

Though the inheritor of wealth, and himself the creator of added wealth, he was his own master. His abundant possessions found him, and left him, free. His soul lost no freshness, no grace, by reason of wealth's vain-glory. He felt the obligation and the honor of Christian stewardship. He gave gifts quietly, joyfully, like a prince. His benefactions were constant and large. The law of the tithe was not enough for him. He measured his privilege by the greatness of the need and the extent of his resources. With his gifts he gave himself.

After a long illness, the death of Mr. Dodge occurred at his summer home in Bar Harbor, Maine, August 9, 1903.

Marcus Baker.

Marcus Baker, the Assistant Secretary of the Carnegie Institution, performed his last work in editing the present volume. He had been in failing health for some months, and the end came while the book was on the press. He was a scholarly man, of broad culture, and talented in many fields; he was a conscientious, painstaking, accurate man, doing thoroughly and well that which he undertook;

and he was an optimistic and affable man, who delighted to be of service to others. His activities were so varied and his responsibilities so numerous that the Carnegie Institution is but one of several organizations to suffer by his loss; yet the Institution will miss him most, for he had assisted in its organization and knew all the details connected with its affairs. He was a man of science, occupied chiefly with geographic and bibliographic researches, but a contributor also to history, and a lifelong student of mathematics. Though possessing no capital and not engaged in business in the ordinary sense, he yet held several positions of trust in business organizations. He had also completed a law course, and was competent for admission to the bar. An outline of his life is given below.

He was born at Kalamazoo, Michigan, on the 23d of September, 1849, the earlier part of his youth being spent on a farm, and the later in the city of Kalamazoo. His education began in the common schools of Michigan. Two years were spent in Kalamazoo College and two in the University of Michigan, from which he was graduated in 1870. The degree of LL. B. was received from Columbian University in 1896.

In the summer after graduation he assisted Professor Watson, of Ann Arbor, in computations for the *Nautical Almanac*. Then for a year he held the chair of mathematics in Albion College, and for two years was instructor in mathematics in the University of Michigan. He then availed himself of an opportunity to enter the United States Coast Survey, and was a member of that corps for thirteen years. He assisted Dr. W. H. Dall in surveys of the coast of Alaska, having for his special function the astronomical determination of latitude and longitude. Afterward, at the offices of the Survey in San Francisco and Washington, he aided in the preparation of the *Coast Pilot of Alaska* and of a bibliography of the geography of Alaska. In later years he compiled for the Geological Survey a dictionary of Alaskan names. In 1882 he was sent to Los Angeles, California, by the Coast Survey to install and conduct a primary magnetic station or observatory, and he was afterwards assigned to an investigation of the tides and currents of New York harbor and their relation to the coastal bar and other shoals.

In 1886 he resigned to accept a position in the United States Geological Survey, and he was connected with that organization until the founding of the Carnegie Institution. For a number of years he had charge of the northeastern topographic division, supervising the mapping of Massachusetts, Rhode Island, Connecticut, and a

part of Pennsylvania. He was afterward editor of topographic maps. While these were his principal routine duties, they occupied only a portion of his time. He was from time to time entrusted with various special researches, usually of a literary or bibliographic character, for the purpose of aiding the director in the preparation of special reports and other documents. He represented the Geological Survey on the Board on Geographic Names, and for more than ten years was the secretary of that board, having charge of its files and collating the recorded usage of most of the names submitted to the board for decision. He was also the editor of its bulletins.

When a commission was appointed by our government to investigate the matter of the Venezuelan boundary, Mr. Baker was employed as geographic expert, taking leave of absence from the Survey for that purpose. He prepared a compendious report, including an exhaustive bibliography of the maps bearing on the boundary dispute. Afterward, when arrangement was made for arbitration, he was employed by the counsel for Venezuela, and spent two years on the preparation of the case.

Mr. Baker was member of a number of scientific societies, from which he accepted duties that occupied much of his leisure. For several years he was secretary of the Philosophical Society of Washington and editor of its *Bulletin*; and afterward he was its president. He was an officer of the Geographic Society from its organization, and also of the Historical Society. When the scientific societies of Washington became affiliated through the constitution of a joint commission, he was chosen its secretary, and in that capacity began the preparation of the joint directory of scientific societies, which he continued from year to year. In the same connection he was made secretary of the local committee for the entertainment of the American Association for the Advancement of Science in 1891. When the joint commission was succeeded by the Washington Academy of Sciences, in 1898, Mr. Baker was chosen not only to the Academy but to its board of managers, and he afterward became the editor of its *Proceedings*.

Mr. Baker in 1874 married Sarah Eldred, who died in 1897. In 1899 he married Marion Una Strong, who, with two children, survives him. His death occurred December 12, 1903.

ACCOMPANYING PAPERS

CONTENTS OF ACCOMPANYING PAPERS.

REPORT OF COMMITTEE ON SOUTHERN AND SOLAR OBSERVATORIES

CONTENTS

The undersigned, members of a Committee appointed to consider certain large projects in astronomy, have given close attention to that duty and herewith present the conclusions reached. Other matters still pending or requests from the Trustees may necessitate further communications which we shall take pleasure in presenting as occasion may arise.

I. General Recommendations.

We strongly urge the adoption of each of these projects submitted to us for examination, so far as this can be done consistently with other obligations which the Carnegie Institution may feel bound to assume. Our recommendations refer to definite programs of scientific work to be accomplished and not to the establishment of permanent institutions. Should these programs be carried to a successful issue, we are aware that in the meantime other demands for astronomical work quite as important and not less pressing may arise. The question whether the Carnegie Institution shall consider it advisable to take them up is one which we will not attempt to discuss at the present time.

For the present we urge the establishment of an observing station in the southern hemisphere for the accomplishment of certain definite works of observation, so arranged that their completion may be anticipated within a period of ten or twelve years from the time of beginning.

We also urge the establishment of an observing station for solar investigation in exceptionally favorable conditions of atmosphere, to be kept up during one full sun spot period at least (eleven years), or, preferably, through the maximum which may be expected to occur about 1916. In connection with this station (or with both stations) we recommend the construction and maintenance of a powerful reflecting telescope, as large as it is thought prudent to undertake, for use in astrophysical investigations upon the stars.

In appropriate sections of this report and in the appendices the several projects are discussed in some detail. We are indebted to our scientific colleagues for important advice and suggestions upon the matter herein treated, for which our best thanks are due.

These projects involve a large expenditure of money and scientific effort. Their importance demands that the reasons justifying an expenditure so great should be stated somewhat in detail, and that the character and importance of the scientific works for which provision is desired should be indicated.

Before proceeding to these details, however, it may be well to take up some of the general considerations that have a bearing on these projects.

Under its policy, as we understand it, the Carnegie Institution would not be inclined to establish an astronomical observatory for the general promotion of astronomy without more definitely expressed object. In lieu of special instructions the Committee looks for guidance to the declared policy of the Trustees as found in the Year Book for 1902 (pp. xxxvi–xxxvii). From this it appears that:

> "The Institution does not propose to undertake—
> (*a*) To do anything that is being well done by other agencies.
> (*c*) To enter the field of existing organizations that are properly equipped or are likely to be so equipped."

Aside from the support which it is the policy and practice of the Carnegie Institution to extend to investigations to be carried on through existing agencies, it desires

> "To promote original research by systematically sustaining—
> (*a*) Projects of broad scope that may lead to the discovery and utilization of new forces for the benefit of man.
> (*b*) Projects of minor scope that may fill gaps in knowledge of particular things or restricted fields of research."

These declarations seem to mean that the Instituution desires especially to assist in repairing notable deficiencies in knowledge. There is a kind of balance in the progress of various branches of research which it is desirable to maintain, so that results in one line of inquiry shall not remain too long unused in the archives of science while another line of investigation is bringing what is necessary for their proper interpretation. A virtual deficiency in knowledge may also be created through the neglect of opportunity to undertake some promising line of investigation, caused either by the forbidding cost of preparation or by some special inconvenience attached to its prosecution.

It will not be difficult to show, under this interpretation of the policy of the Trustees, that the projects considered here are singularly adapted to the support of the Institution.

Drawbacks to be considered.—It is proper, however, that we should call attention to one of the drawbacks which might result from the establishment of either of these observatories upon a temporary basis. The withdrawal from their present relations of a large number of competent astronomers for temporary duty with these observatories must be productive of some degree of disorganization in those institutions from which they may be recruited. The return of these men to more permanent relations may also be attended with more or less uncertainty, in view of which it is obvious that the compensation of the astronomers required for temporary duty must be decidedly larger than would be necessary under other conditions.

Another disadvantage of a temporary as compared with a permanent organization is the large proportion which the expense of installation bears to the total expenditure involved, together with the fact that the termination of the programs proposed would leave this expensive equipment idle on the hands of the Institution, unless some further provision should be made for its useful employment. This further use, however, could be effected in either of two ways:

(1) By provision for further employment on the part of the Institution.

(2) By donation or sale of the instruments to existing institutions which are in a position to make good use of them.

Relation to existing institutions.—The Committee desires also to record, in the most explicit manner, its opinion that nothing which may be determined in relation to large projects in science should be allowed to impair the total amount of support which the Institution

now extends in aid of astronomical research at existing institutions, so long as the results obtained shall show that such aid is efficiently expended. By the encouragement of several active centers of astronomical activity the amount of valuable astronomical product is stimulated out of all proportion to the means expended, and the future development of a number of able astronomers is made more certain than would be the case in a policy of greater centralization.

PROPOSED OBSERVING STATION IN THE SOUTHERN HEMISPHERE.

The project for a Southern Observatory was advocated in the general report of the Advisory Committee on Astronomy for 1902. Among other things, the committee says:

"The third point which has specially impressed itself upon our attention is the great deficiency of observatories in the southern hemisphere. * * * Since more than one quarter of the entire celestial sphere is efficiently reached only from the southern hemisphere, it is obvious that there is now very great disparity of astronomical resources to the disadvantage of the southern hemisphere. * * * We regard this question to be exceeded in importance only by the urgent need of provision for current work to which we have already alluded." (*Year Book* for 1902, pp. 89–90.)

The matter was further discussed in Appendix A and elsewhere in the reports of the committee.

We advocate the establishment of another active astronomical station in the southern hemisphere because there is needed in certain special lines a much greater output of astronomical observations, which can be obtained only by means of an observatory in some part of the southern hemisphere. This need veritably exists, as we shall attempt to show. It has grown out of the present progress of astronomy. The satisfaction of this need seems possible only through the aid of the Carnegie Institution.

What, then, is this need? We attempt to answer this question more fully in the section of this report devoted to special consideration of the proposed Southern Observatory; but we may be permitted to touch upon it briefly in another way here.

For three centuries astronomy has been developing by a rapidly increasing ratio of progression in attack upon the most accessible sources of knowledge. Until after the middle of the nineteenth century it was almost wholly absorbed in

(*a*) The study of the shape and dimensions of the earth and other terrestrial problems;

(*b*) An investigation upon the motions of the bodies associated together in the solar system.

The problems encountered in these two lines of research are of singular fascination, and they are capable of development in essential points upon the basis of observations that we now regard as comparatively crude. The element of time was favorable. Observations extending over relatively few years are sufficient to develop the facts of planetary astronomy in their broad outlines. It was natural that these subjects should have received the first attention of astronomers. The number of separate objects concerned was small, so that the work of observation did not bear a relatively large proportion to that of mathematical development of them.

But the time came, during the last third of the nineteenth century, when astronomers began to feel satiated with their conquests in the terrestrial and planetary fields. Facts and observations relative to the astronomy of the stars had been slowly accumulating. Plans were made and carried out to increase the extent and value of these observations. The *non possumus* of the more conservative astronomers of the old school fell upon less and less willing ears. The influence of the ideas which led to the establishment of the great Pulkova Observatory, sixty years ago, began to be more and more felt. On all sides it is perceived that the *sidereal problem* is to be the astronomical problem of the twentieth century, as the planetary problem was the characteristic problem of the eighteenth century.

Thus a great movement has been inaugurated for the accumulation of facts and observations in sidereal astronomy. The scope and meaning of the movement may be learned from a brief review of a part of the work accomplished or in progress :

(1) We are witnessing now the greatest activity ever known in the history of astronomy for the accurate, systematic, and extensive measurement of the positions of the principal fixed stars at successive epochs.

(2) We have seen the plan of the *Astronomische Gesellschaft*, for the accurate observation of all stars down to the ninth magnitude in the northern heavens, brought to the point where its success is fully assured. This is by far the most extensive work in astronomy ever attempted, previous to the project for the Astrographic Chart next to be mentioned.

(3) We are now watching the successful prosecution of observations for the Astrographic Chart—the attempt to measure from photographs the accurate positions of all stars down to the eleventh,

and to chart all down to the fourteenth magnitude. This gigantic project has been taken up with a degree of faith and a force of determination which proves the widespread interest that is now felt in sidereal problems.

(4) There is remarkable growth in the number of successful attempts to measure the motions of stars in the line of sight. Twenty years ago it seemed almost impossible to make these measurements with sufficient accuracy; now, at a few observatories it has become almost a matter of routine; and seven or eight of the largest telescopes in the world are dovoted in part to this work.

(5) Until comparatively recent years the measurement of the distances of stars was taken up here and there; but rarely did a single astronomer attempt to make this measurement for more than two or three stars. Now this work is carried on successfully for series of stars by observers who are devoting many years to it.

All of these works, and many others not here enumerated, have been undertaken for the purpose of throwing light upon the astronomy of the stars, in and for itself.

But activity in these and similar lines is almost wholly confined to the northern heavens. The resources required for extending these researches over the southern sky are wanting, except for the Astrographic Chart. Even for that it is a question whether the present or prospective resources of the southern hemisphere will prove sufficient.

Certainly there is no one subject in physical science that seems better entitled to command some part of the interest of every intelligent man than that which relates to the structure and mechanism of the vast aggregation of stars and nebulæ which challenge the curiosity of all beholders. The scale upon which the visible universe is constructed, and the inconceivably rapid as well as perplexing motions which prevail among the bodies that it contains, propose to our minds problems which have a high degree of interest, both physical and philosophical. The nature of these problems we will touch upon more fully in a subsequent section of this report; and we shall there endeavor to show that there are problems which justly concern astronomers of the present generation. There are some secrets of Nature which may forever remain concealed from the eye of research. We do not know. But problems which concern geometrical relations and motions can be solved when time and opportunity are propitious. From what has been already learned about the structure and mechanism of the universe of stars, it is easy to see that very much more must be very nearly in sight.

Now, precisely the greatest obstacle to a clear view of the stellar problem is the comparative lack of information about the stars in the far southern sky that are invisible to northern observers. When a sufficient number of accurately observed facts concerning these shall have been obtained, research upon the sidereal problem will have received a strong impulse forward. If for any reasons it is desirable to know the accurate distance between two points, no expenditure of labor and skill upon the measurement of three quarters of that distance is of full effect if our knowledge of the remaining fourth depends upon a rough reconnoissance. The observational situation in the southern hemisphere is fairly illustrated by means of this comparison, which also illustrates how the execution of the proposed observations in the southern hemisphere would increase the value of what has been already accomplished by northern observers.

In the directions where we were most likely to obtain trustworthy information as to the probabilities we have made careful inquiry to ascertain whether there is any present likelihood of increase in astronomical activity in the southern hemisphere through existing agencies, and we have not only failed to learn of any such likelihood, but, as will be shown, it has become clear that the present and prospective resources of the southern hemisphere are scarcely adequate to the fulfilment of existing obligations, for which we had supposed that adequate provision had already been made.

The works of observation that we propose could probably be brought to a successful conclusion within ten or twelve years from the time of beginning. While we are confident that the necessity for maintaining the proposed observing station for a still longer period after the expiration of its first mission would be as keenly felt then as now, that will be a question for the future to decide in the light of experience. Funds of the Institution need not be tied up for this purpose in perpetuity.

Thus we have in the proposed Southern Observatory two important requisites that would seem to commend it to the favorable attention of the Carnegie Institution :

(*a*) A project of broad scope, embracing also features of minor scope, that may lead to important discoveries and that will certainly "fill gaps in knowledge of particular things."

(*b*) It does not "enter the field of existing organizations," and does not propose "to do anything which is being well done by other agencies."

In the section of this report which deals with the proposed Southern Observatory various definite works are enumerated with particular care, and reasons are given in each case why it is important that they should be executed at this time. An effort has been made to indicate the bearing of these works upon the current of astronomical research, proving the manifold benefits which are certain to accrue to a numerous class of investigations. Should this scheme be carried out to a successful conclusion, it will hereafter be found that the results will become interwoven with the progress of knowledge. The points of contact and of essential support will become numerous beyond the power of present estimation.

What are the chances of success in carrying out the project should it be undertaken? This is certainly a pertinent question. We shall not attempt any over confident prediction in this direction. Much depends in a matter of this kind on the manner in which the enterprise is conducted, and also upon the point whether the means are secure and proportioned to the end; but we are able to say with confidence that there would be few obstacles in the way of success. The importance of the various works is incontestible; this should inspire those who are to execute them. The observers need not be troubled by the least suspicion of the futility of what they are doing. The success of the work does not depend upon the success of hypothetical experiments. It is not proposed to do anything by a process so novel that the guidance of experience in any material degree will be wanting. Even the probable duration of the works can be quite accurately gauged through the teachings of abundant experience. From the outset, in all really essential features, the observers will know what they ought to do and how they ought to do it.

The questions of organization and choice of site for the proposed observatory in the southern hemisphere present some problems which call for careful thought. The project admits of various scales of expenditure, but it is earnestly to be hoped that the scale adopted by the Institution would be sufficient to cover all of the works recommended.

Proposed Observing Station for Solar Investigation.

Among the projects commended by the Advisory Committee on Astronomy in 1902 is that for the establishment of an observing station for solar research at an elevated point where the atmospheric conditions should be of exceptional excellence. Closely associated

with this project and equally commended to favorable attention was another project for the construction and maintenance of a large reflecting telescope to be used in stellar researches.

In the domain of astrophysics there appears to be no direction in which there is a more hopeful prospect of a marked advance in knowledge to follow a large investigation wisely planned.

It is not surprising that the nature of the sun should have been an object of speculation from the earliest times. In the estimation of mankind generally the sun seems to be obviously the most important of the celestial bodies. From the astronomer's point of view, however, the sun has another interest which is not less intense and not less impressive.

The sun is a star. It is the only star near enough to us to permit of detailed study into its physical constitution. The next nearest star of whose existence we are now aware is nearly three hundred thousand times more distant than the sun. It should not be difficult to understand that if we wish to study the physical condition and history of stars under such difficulties of distances, it would be an immense gain if we could attain detailed knowledge as to any one of them which might happen to be near. The sun offers this opportunity.

It is already known that a great variety of successive stages of development seem to be illustrated in the analysis of the spectra which the stars present. There appears to be no link missing from the hottest and brightest to the coldest and least luminous. In the study of this development, then, it is manifest that we should gain a great advantage—the key to the position—if we could thoroughly know the physical constitution and tendencies of one in this series of stars. The sun is the one which offers this opportunity.

But astronomers have sought to take advantage of this opportunity during three centuries. For the last fifty years an immense total of effort has been expended on this research. For more than forty years the spectroscope has been in the hands of astronomers as an effective weapon of solar research. There are numerous institutions where solar research is now actively carried on. Where, then, is the field which would justify a special effort on the part of the Carnegie Institution to enter upon solar research at the present time?

The expanding record of our knowledge relating to the physical constitution of the sun is parallel with that of increase in the power of the appliances used in this research.

First, we have the improvement of the telescope. In its application to this purpose, we believe that this improvement has by no means reached its limit.

Second, we have the development of spectrum analysis—the study of light radiations in the spectrum. Rapid improvement in the apparatus which this method employs has been continuous for nearly forty years, and seems to be more strongly marked now than ever before. This is well illustrated in the recent development of the spectograph and the spectroheliograph, and in the marked advance brought about in the size and quality of diffraction gratings.

Third, there has been developed within the last quarter of a century the power of accurately investigating the distribution of heat radiations in the spectrum of celestial objects, and especially in that of the sun. An immense advance in knowledge has been recorded in this way through the accurate representation of a large portion of the solar spectrum containing rays invisible to the eye and beyond the reach of the photograph.

These facts alone might not be sufficient to fully warrant the project of combining in one large establishment the harmonious cooperation of these three methods of research developed to the highest state of science at the present day, but they should certainly be sufficient to excite interest in the question.

The present state of our knowledge of the sun is surprisingly deficient in comparison with that which would seem to be attainable by the use of the most powerful appliances. The fact that these appliances have not been brought to bear in anything like the extent demanded by needs well understood affords no very hopeful outlook in the immediate future that these needs will be met at existing institutions. What immense additions to knowledge are possible through the intelligent invention and use of powerful instruments of research in this line is well illustrated in the normal solar spectrum produced by Rowland less than twenty years ago, by methods remarkable for their simplicity, which at once superseded all the laborious efforts in that direction resulting from the use of less powerful apparatus during the preceding thirty years. We believe that similar opportunities for progress in solar research are within the reach of investigators of the present day through the provision of apparatus of which the possibilities are now generally conceded.

A distinct advantage might also result in the attempt to carry on in one institution the three related methods of investigation—tele-

scopic, either visual or photographic; spectroscopic, involving spectrum analysis in its various forms; and bolometric analysis of heat radiation. These may be likened to three senses by which solar phenomena can be apprehended and described. One of these senses may become aware of some passing and suggestive phenomenon in regard to which the evidence of the other senses will be immediately required. There would appear to be a strong likelihood that a great gain will be experienced in the study of local developments on the surface of the sun simultaneously from three points of view.

The chief obstacle to progress, however, appears to reside in atmospheric disturbances which prevent the full advantages that otherwise might be realized from the use of powerful optical appliances. There is scarcely any department of astronomical research wherein this difficuly is more acutely felt. Very naturally nearly all astronomical institutions founded by governments and universities have been located in or near capital cities or other great centers of population, where the manifest advantages and stimulus of a scientific atmosphere may be obtained. There is doubtless the desire also to maintain the visible connection between them and the power by which they were created. The observatories of Paris and Berlin are in the heart of great cities. In the case of very few existing observatories can it be said that the choice of site was influenced in any marked degree by a consideration of superior advantages in atmospheric conditions.

But superior atmospheric conditions are precisely what solar research most requires. Where these do not exist, small telescopes frequently answer almost as well as large ones. The air is in an almost continuous state of agitation, which prevents the employment of high optical power. Dust and smoke absorb a portion of the solar radiations, and these in an unequal degree. If we are to establish a solar observatory in ideal conditions, we must seek to avoid these disadvantages. While we shall not be able to accomplish this perfectly, we ought to inquire what gain may be fairly within reach.

Although the Carnegie Institution is free to locate a solar observatory anywhere in the world where this can be done to the best advantage, we have still thought it likely that the proper balance of advantages can be best secured within the United States. For some purposes a subtropical station might offer peculiar advantages for certain sections of this research. If the observatory had but one thing to do, and if the methods of observation could be formu-

lated and reduced to routine in advance, the arguments in favor of a subtropical station might be stronger than they are; but we know of no instance of the development of an important research outside the temperate zones. One of the highest obligations of the proposed observatory would be to follow up and take advantage of the indications brought out by successive programs of observations. Interpretation of results and choice of new and promising lines of attack would probably be the yearly experience of such an observatory, even though the main current of its work would consist of comparative observations maintained through a long period.

In order to secure the greatest freedom from atmospheric disturbances and from the absorption of solar radiations by the atmosphere, one might suppose that an extremely high elevation, where the observer would have below him as much of the atmosphere as possible, should be chosen. How great this elevation ought to be is involved in more than one consideration. The cost of maintenance would always increase very rapidly with increasing altitude. This becomes an important obstacle where a large observatory is in question. At elevations much above 10,000 feet extremes of temperature are likely to prevail, hurtful alike to the action of the instrument and to the observer. The rarity of the atmosphere in producing mental enfeeblement and prostration is a most serious drawback at extremely great elevations. No doubt men accustomed to sea level conditions can also live at altitudes exceeding 15,000 feet, if they have nothing in particular to do; but if they are to be called upon for prolonged and intelligent exertion, the case is different. Furthermore, it is quite unlikely that the most favorable atmospheric conditions are to be found above a certain moderate elevation.

These considerations moved us to seek an elevation of from 4,000 to 6,000 feet for the main observatory, with the idea of selecting a suitable auxiliary station at a much higher elevation for certain observations not requiring great steadiness of atmosphere, but needing a transparent atmosphere.

Our preliminary examination of climatic conditions seemed to indicate that a suitable station could be found in California. The steps which led to the provisional selection of Mount Wilson, a few miles east of Los Angeles, as the main station for the proposed solar observatory are detailed in the section of this report appropriate to that subject, and in the report of Mr. W. J. Hussey (Appendix A), who was employed to make the requisite explorations and atmos-

pheric tests. The atmospheric conditions on Mount Wilson at an altitude of 6,000 feet, as reported by Mr. Hussey, appear to be remarkably fine, and this opinion is supported by Messrs. Campbell and Hale, who took part in the final tests.

Auxiliary station for solar observations.—Among the problems of the proposed observatory would be that of determining what is the total amount of heat radiated by the sun and to what extent, if any, this amount varies—the determination of the so-called solar constant. For this purpose it would be ideal to make this measurement from a point which receives the radiation of the sun without any absorption whatever by the earth's atmosphere. As an approximation to this, one might choose a station upon the top of the loftiest mountain which can be ascended; but for reasons already stated we are not in favor of this extreme elevation.

It has been thought that some such site as Mount Whitney, about 170 miles north of Mount Wilson, might offer a suitable auxiliary station for the special observation of heat radiations in relation to the solar constant. The altitude of this mountain is nearly 15,000 feet. This is an elevation beyond which it can scarcely be supposed that continuous observations could be effectively carried on. In 1881 Professor Langley occupied this peak for a similar purpose. We think it might ultimately prove that after occupation for two or three seasons such a relation with the results obtained on Mount Wilson might be established as to render the continued maintenance of such an auxiliary station unnecessary. The principal study of the heat radiations of the sun would then be made at the permanent station on Mount Wilson, or wherever the main station might be located.

In the section of this report devoted to the proposed Solar Observatory will be found a presentation of reasons for our conviction that this institution should be established, together with a carefully prepared description of the appliances that would be required.

Advantages of a great reflector.—As connected with both the Southern and the Solar Observatory, and a consistent part of them, we think that a large reflector should be mounted and maintained for the investigation of stellar spectra. From what we have already stated, it will be seen that we regard the relation of solar physics to stellar physics the most important inducement for the establishment of the Solar Observatory, though not the only one. In order to make a comparative study effective, there would be a great advan-

tage in associating the two branches of study, solar and stellar, in one establishment. But, aside from this, we think that the site which has been provisionally selected (or a still better one if it can be found) would be most admirably suited as the location for the most powerful telescope that can be constructed. The occupancy of this site for such a purpose seems to offer a rare opportunity that ought not to be neglected.

Furthermore, it is now conceded that the construction and successful operation of a reflecting telescope, having a mirror five feet in diameter, is entirely feasible. We have the benefit of experience, both at the Lick and Yerkes observatories, in demonstrating the great advantages to certain researches in astronomy which may be derived from the use of large reflectors. We feel no hesitation in offering the prediction that researches in astrophysics would be enriched to a remarkable degree by the use of the largest reflectors that can be made, and we would strongly urge the advisability of providing a very large reflecting telescope as part of the equipment of the proposed observatories.

Policy of proposed Solar Observatory.—Plans and estimates for a Solar Observatory, and likewise for a Southern Observatory, have been prepared upon our understanding that the Carnegie Institution is not in favor of establishing permanent observatories, but that works undertaken must be in the nature of expeditions for the solution of definite problems, in limited periods of time. In some ways this policy seems to us to have advantages, especially in relation to problems for whose solutions the instruments are of standard form and for which the methods have been perfected and thoroughly tested. Examples of this are meridian circle, heliometer, and micrometric work generally. On the other hand, this policy is not a natural one for those new works which call for developments in instruments and methods. Some portions of the solar work and the application of photography to parallax problems are examples of this.

The task proposed for the Solar Observatory is only in part of a routine character. A large part of its work ought to be one of development, in relation to which it would be difficult to foresee, in all its details, the exact character of the equipment which might be best calculated to meet successive requirements of investigation as they may arise. Consequently, we are of opinion that the director of the proposed observatory should not be hampered with a rigid program of method and equipment, prescribed in advance.

Problem of Organization.

It seems almost superfluous to add that the prompt equipment and organization of two observatories such as we have recommended might be a temporary strain of some severity upon the existing resources of astronomy. The only remedy for this which we can suggest is the avoidance of undue haste. There can be no question that the success of these enterprises would depend very largely upon the ability and energy of the agents selected to carry them out. It may safely be said that no enterprise in science is so important that it cannot afford to await the appearance of scientific men of sufficient foresight to appreciate its importance and of sufficient ability to carry such an enterprise to fruition. On the other hand, we think that it would be far more logical for the Institution to determine from the consensus of astronomical opinion what is best to be done in astronomy and then to take means to get that thing done, than it would be to build and equip an observatory with instruments and staff in the hope that such staff might find something profitable to do. In the former case it is but a question of time in finding the astronomer, previous to which no expenditure is lost; in the latter the expenditure is incurred at the outset and remains to be justified. The latter course is unavoidable in the case of governments and institutions which establish permanent observatories, and on the average it produces good results; but the Carnegie Institution enjoys the rare privilege of taking the former course, which we believe to be the safest where it can be employed.

II. Proposed Observatory in the Southern Hemisphere.

In inviting the attention of the Trustees to the project for an observing station in the southern hemisphere, we are not proposing a mere generality. We propose certain definite things to be accomplished.

This undertaking essays the humbler task of producing the facts of observation by means of which future hypotheses must be tested and upon which theories must be founded, rather than the more brilliant role of attempting some great scientific generalization; yet it does not altogether lack the attractiveness which belongs to great discoveries, since it is intended to prepare a road by which discoveries can be reached. In any future division of honors it cannot fail to win its share. In fact, it is the more sure of this because theories often fail and are replaced by more perfect deductions; but the observations always remain, partakers in final success. The feasibility of the works here proposed is undoubted. Their value can be safely predicted. This value will be enduring, not temporary.

But this project does not propose to build for posterity alone. It promises to lead to generalizations of immediate importance, and if the valuable deductions to which it may be fairly expected to contribute at once should fall short of expectation, it could only be a question of a few years before these could be realized.

The enterprise herein proposed offers a peculiar attraction in the powerful alliances of which it would form a part. It would form one of the converging lines which must result in producing what will prove to be the characteristic advance of astronomy in this century.

It will be natural first to consider the nature of the general problem in astronomy to the solution of which the establishment of the proposed observing station in the southern hemisphere may be expected to contribute.

Problem of the Sidereal System.

The mere appearance of the starry sky at night cannot fail to impress the reflecting mind with the thought that this vast aggregation of stars must contain within itself the evidence of organic arrangement. A more attentive examination, even without a telescope, while it reveals a general uniformity in the distribution of the stars

of various orders of brightness, also makes us aware that there are clusters of stars which point unmistakably to the operation of law. The vast aggregation of clusters making up the nebulous belt known as the Milky Way, which spans the sky as a kind of celestial equator, suggests orderly arrangement.

When we assist our researches with the telescope, merely as an optical appliance, evidences of such arrangement multiply until we are finally led to conjecture, by statistical methods alone, that the earth is situated within a vast cluster of stars very much more extended in the direction of the Milky Way than in other directions.

If we fortify our telescope with means of measurement we shall discover, after sufficient lapse of time, that some of the stars are in motion relatively to others. As we persevere in our measurements we shall discover an ever increasing number of stars partaking of this motion ; and we shall finally conclude that all the stars are in motion, some in one direction, and others in another. Some of these motions are only apparent. Disentangling these, after immense effort, we shall be able to recognize a peculiar drift of the stars, precisely like that which appears in surrounding objects when we are moving rapidly among them. This will finally prove to us that the sun is only a star, and that it is in rapid motion like all the others. Later we shall notice that separate groups of stars seem to be moving in a common direction like swarms of meteors ; and we shall begin to suspect that other evidences of law in these motions may be revealed to us at any moment if we persevere in our investigations.

Parallel with the discovery of these facts we shall be learning something of the distances of the stars. We are oppressed with the conviction that, so long as we are studying the arrangement and motion of the stars as objects upon a map, our information will be lacking in a vital element. We long to gain some conception of the space relations of the stars. From the very first we will have been led to suspect that some stars are brighter than others because they are nearer. When we attempt to test this hypothesis by measurement, we shall encounter the most formidable difficulties. Putting this difficulty in its simplest form it will mean that, if the vast area of the earth's orbit, more than one hundred and eighty million miles in diameter, were to be brightly illuminated and removed to the distance of the nearer stars it would appear as a mere point in the most powerful telescopes ; and to discover its dimensions would require the greatest refinement of skill even in the more favor-

able cases. Nevertheless, we shall be able to verify our conjecture in part, though we shall find that motion rather than brightness is the better criterion as to the distances of stars. Later we shall discover that we can test the relative distance of one large aggregation of stars from another, through one of the generalizations resulting from an investigation of the solar motion. Now, we shall have arrived at the point when we shall feel that we may some time see the stars in space of three dimensions—when we shall not only be able to make a flat picture of their relations as they appear to the eye upon the celestial vault, but shall also be able to construct a model showing the special and general relations in distance in space of three dimensions. Astronomy is standing at this point now. It takes some pride in what it has accomplished in this line already; but it is also more dissatisfied than ever with the amount of accurate knowledge which it has, because it sees how comparatively easy it would be to increase this knowledge in a very large ratio.

Again, parallel with these researches upon the motions and distances of the stars, there has been growing in importance another class of inquiries. Arming the telescope with a new appliance, the spectroscope, the evidence of universal motion among the stars, whether toward or away from the sun, has been most brilliantly confirmed. In this way the solar motion has been verified, and the approximate velocity of the sun in its flight through space has been determined.

Through its power of physical and chemical analysis the spectroscope has brought to light a collection of new facts of the highest importance in their bearing upon cosmical problems. It has shown the presence of terrestrial substances everywhere among the stars. It has demonstrated anew and more convincingly that the sun is a star, and even that there are multitudes of stars closely resembling it in their physical condition. It has shown us something of what is going on in the development of suns, or stars, revealing them at all stages of evolution and connecting one with another by imperceptible gradations from the newest to the oldest forms.

The unity of the stellar universe is thus demonstrated from three distinct points of view, and the conviction is brought home to us that there must be within our reach further and more interesting facts which will illustrate something of the arrangement and mechanism of this vast system of worlds flying hither and thither through space.

In outline this is what we shall term the *sidereal problem*, which

is now coming into view as the problem of the time, surpassing in the universality of its interest and in the grandeur of its scope all the physical inquiries which engage the attention of scientific men.

This problem should not be regarded, however, as synonymous with the whole range of sidereal astronomy. In the sense in which the term sidereal problem is employed here it would include those classes of research the chief interest of which centers in their bearing upon the description of the stellar system as a unit. There is another large range of extremely interesting investigations in the stellar field, in relation to which the chief interest centers in the natural history of individual objects. Even these may turn out to have their bearings upon the sidereal problem as a unit, but the connection is not so apparent. However much one class of researches may blend into another, the distinction here made may be found a useful one in the effort to give unity of design to the aim of the proposed Southern Observatory.

To unravel the problem of the solar system was the task which was first seriously proposed to astonomers three hundred years ago. Galileo, Kepler, Newton, and a long line of distinguished successors have successfully grappled with the intricate questions involved in that problem. Now the aspirations of astronomers are reaching beyond the boundaries of the solar system into stellar space. The stellar problem confronts us as the serious occupation of the present and the future, and it is illimitable in extent.

Need of More Astronomical Observers in the Southern Hemisphere.

Why should we go to the southern hemisphere in order to work upon this problem?

Were our object planetary research the inducement for another southern observatory might not be sufficient. All the planets can be seen from either hemisphere, and from either the whole fabric of planetary astronomy could be constructed without help from the other, though there would be undoubted advantage in cooperation.

But in stellar observation the case is different. Nearly one-quarter of the entire celestial sphere is inaccessible to exact observation from the observatories of the northern hemisphere. If we need observations of the nebulæ and stars in that part of the sky, we must go to the southern hemisphere to get them. For many of the most delicate researches fully one third of the sky should be under observation from the southern hemisphere.

Why cannot the observatories already in the southern hemisphere make all the observations that are needed there?

For this duty in the southern hemisphere we have about one tenth of the force which is available in the northern hemisphere for such observations—one tenth of the force to do one quarter or one third of the work. This point might be enlarged upon in detail, but it is so notoriously the fact as to make such illustration superfluous. The unanimous verdict of astonomers on this point may be derived from the correspondence which is transmitted with this report.

Nor does there appear to be any prospect now or within a reasonable time in the future that any material addition to the astronomical forces of the southern hemisphere can be anticipated through existing agencies. This ought not to be a matter of surprise. In addition to the numerous private, institutional, and municipal observatories in England, several astronomical establishments, including the celebrated Greenwich Observatory, are supported by the government in the British Islands. Therefore the British Government, in maintaining an astronomical observatory of high rank at the Cape of Good Hope, can scarcely be criticised for not doing more in that quarter of the world. Great Britain is the only one of the great powers from which support of astronomy in the southern hemisphere can be anticipated. Consider the local resources. We have the English colonies in Australia. Three small observatories are maintained there, of which, until recently, two have been almost exclusively concerned with meteorology. For somewhat more than forty years astronomical work has been carried on in the observatory at Melbourne, but the tendency recently has been to curtail this. Furthermore, the three Australian observatories have each recently undertaken a share in the Astrographic Chart, which is practically certain to absorb their entire energies for many years to come. Nor should the Australian governments be criticised because they maintain only three observatories on a small scale when we reflect that New South Wales, Victoria, and Western Australia combined have a population smaller than that of Texas. Still less should we anticipate any important contributions from the comparatively small communities in New Zealand, Tasmania, and South Africa. There remain only the governments of South America, and of these only two, Chile and Argentina, are in a geographical situation to become factors in consideration. Each of these countries maintains a national observatory. In the period from 1854 to 1862 the National Observatory of Chile, under a German astronomer, was active in a small

way. Recently a share in the Astrographic Chart was undertaken there, but Director Loewy informs us that this work has not been prosecuted there, and he expresses the hope that the Carnegie Institution may supply the vacancy. A strong national observatory was organized at Cordoba, in the Argentine Republic, in 1870, under the direction of Dr. B. A. Gould. This ranked among the leading observatories of the world for many years; but the financial disasters of Argentina have had a depressing effect upon that observatory, which, though it still continues its valuable work with perseverance and effect, is certainly in no position to undertake any additional obligations beyond that of a share in the Astrographic Chart and the other works there in progress.

Summing up, we find this situation: The Royal Observatory at the Cape of Good Hope will doubtless continue in its functions as an observatory of high rank, with the duty of doing for the southern hemisphere what Greenwich does for the northern as the primary object of its establishment. There planetary and stellar observations will be made, together with the observations that are required in the interest of government surveys. Over and above this we may fairly expect strong contributions from that observatory in a variety of fields. The remaining observatories of the southern hemisphere, with one exception, will be absorbed for at least 10 or 15 years in their work upon the Astrographic Chart, and, so far as can now be foreseen, will not be in a position to undertake much else of an important character. The one exception mentioned is the observatory at Arequipa. This observatory has been occupied in photometric and photographic researches of a nature similar to those carried on at Harvard College Observatory, of which it is a branch. These researches are of great interest and value, but they are outside the scope of the program here proposed.

Why Observations Are Needed in the Southern Sky.

It is desirable to consider more particularly, though briefly, why it is that the observation of objects in the far southern sky is so peculiarly desirable in connection with cosmical problems. If the problem were merely to ascertain whether the stars are in motion at all, or whether their motions vary with apparent brightness, or whether the distances of the stars are measurable in any case, these questions could be, as they have been, settled by observations on objects in the northern sky; but when we reach a point where

there is promise of fruitful generalization, the case may be different. It so happens that the various motions of the stars, real and apparent, are so involved that some means of separating their effects must be devised. Now, it happens that this separation is very difficult when the observations of one hemisphere are considered alone, while there is almost complete elimination of the interdependence of assumptions as to one class of motions upon those as to another when testimony is gathered from the entire sphere. To a very considerable extent this is true of the relation of precession to solar motion.

Again, different forms of a general rotation of the stars have been suggested. It is next to impossible to get any light on this question from the facts of observation relating to one hemisphere alone, because this effect is so entangled with the apparent motions caused by precession and solar motion that they cannot be accurately distinguished. With testimony from the entire sphere, a wrong supposition as to precession or solar motion would not so seriously affect the conclusion as to possible rotation, and in some circumstances scarcely at all.

Again consider the spectroscopic measurement of the velocity of the sun's motion through space. By combining the observations of both northern and southern stars we obtain the motion of approach contrasted with the motion of recession. If there should be anything in the suspicion that all the observations of velocity may be affected with some obscure source of error in common, this would be eliminated in the combination of the two hemispheres. In general it is obvious that we cannot neglect a large fraction of the sky when we are dealing with problems concerning the whole universe.

One consequence of this reasoning is that the proposed program for a southern observatory has this peculiar merit: that the results to be obtained are not only valuable in and for themselves, as constituting a needed increase in the sum of human knowledge, but they increase in a marked degree the value of the facts of observation already obtained in the northern hemisphere.

It does not seem at all surprising that our colleagues, with emphatic unanimity, should have expressed the opinion that additional astronomical effort at the present time can be expended to the best advantage in the southern hemisphere.

We have thus, here, an important research upon which further special investigations are needed, and in reference to which it ap-

pears very improbable that they will receive proper attention from other agencies within a reasonable time in the future.

Works of Observation Proposed and Instrumental Requirements.

We come now to the consideration of the particular works of observation which, in our opinion, could be undertaken to the best advantage by the proposed Southern Observatory. It is neither possible nor desirable that this one institution should undertake to make up all existing deficiencies in astronomical observation in the southern hemisphere. Other agencies have a responsibility in this field. New forces coming to the rescue ought to, and doubtless would, inspire the existing forces of astronomy in the southern hemisphere to still greater effort.

While not attempting to establish an inflexible criterion of selection for works of observation, we are of the opinion that the primary aim of the proposed Southern Observatory should be to make itself felt in the attack upon the sidereal problem as we have defined it—that it should seek to throw light upon the structure and mechanism of the stellar system as a unit. In other respects less attention need be given to those projects of observation that would suffer less by delay.

In preparing the schedule of proposed works we have been greatly assisted by the advice of astronomers whom we have consulted for this purpose. Their letters in response to our inquiries will be found in Appendix B, together with a preliminary statement of works proposed which forms the basis of the comments with which we have been favored. These comments have proved a most welcome assistance, and we have been strongly influenced by them in making up the schedule of proposed works.

(1) *Fundamental Meridian Observations.*

We regard meridian observations of precision, upon the brighter stars, to be of the first importance in any attempt to relieve the situation in the southern hemisphere. In this we are sustained by the nearly unanimous verdict of those whom we have consulted.

We are clear upon the proposition that the exact positions of about 6,000 stars (including all down to the seventh magnitude that are south of —20° of declination) should be determined by fundamental methods, both for the interest which this work commands as an inde-

pendent research and for the bearing it has on other works, including those here proposed.

In this line of work, as in nearly all other departments of astronomy, contributions from the southern hemisphere have always been deficient. With a few notable exceptions, there has been a lack of highly trained and experienced astronomers there. The recent efforts of the Cape Observatory in this line have been both skillful and energetic; but the Cape Observatory alone is unable to offset the numerous observatories in the northern hemisphere engaged in the precise observation of standard stars. The result is that the weight of our knowledge of the positions and motions of the standard stars in the northern sky is fully five times that for the far southern sky. A single corps of observers transferred from the northern hemisphere, where its loss would not be a relatively serious matter, to the southern hemisphere, where its services are so much needed in this line, could reduce the existing discrepancy of weight in fundamental determinations for the present epoch by one half.

It has been conclusively shown that the exactness of our knowledge of general drift in the motions of the stars, whether it arises from solar motion, rotation, or from any other source, depends almost wholly upon the number and precision of our fundamental determinations of the positions of the standard stars. The main battle is fought on this field. Furthermore, astronomy has now arrived at the point where, by comparatively little additional observation, it will be possible to compute the motion of nearly every star brighter than the seventh magnitude in the northern sky with a fair degree of accuracy. With the observations herein proposed, the same thing, in a modified degree, would be true of the stars in the southern hemisphere. Not many more than one third of these stars has been reobserved during the last quarter of a century. This work accomplished, astronomers could hope to deal successfully with problems of motion for all the stars visible without a telescope, and beyond that for all stars down to those which are one third as bright as the faintest visible to the ordinary eye—about 15,000 stars in all. This would give us the first opportunity ever offered for a comprehensive discussion of the solar motion and related problems on a scientifically correct basis, with a liberal supply of material distributed over the entire sphere.

This work would, therefore, possess a high scientific interest as an end in itself; but it would also serve as an indispensable basis for the observation of planets and of the fainter stars. A scheme

of observation like that described further on under (2) would require this work as its foundation. In fact, there is scarcely a single department of precise measurement in astronomy that would not be indebted to this work for a part of the data which it needs.

The problem of observation here suggested naturally divides itself into two sections. The first would be concerned with the observation by fundamental methods, and with the highest precision, of about 600 to 800 of the principal stars; the second would involve the extension of this work by less rigorous methods to about 5,000 other stars—the entire work to be conducted in such a manner as to be systematically consistent in all its parts, and to be a homogeneous whole.

There can be scarcely a doubt that the ideally best result would be attained through the adoption of the transit and vertical circle, for the observation of the principal stars at least. This, however, would prove somewhat more costly in execution, and the completion of the entire work, if it should be carried through with these instruments alone, would be deferred somewhat longer than might seem desirable.

The advantage of economy would attach to the use of a meridian circle for these observations. This would be increased, if the use of such an instrument that has already been thoroughly tested in the northern hemisphere could be procured, and we believe it can. The labor of a thorough investigation of the errors of graduation and other errors of such an instrument is, in itself, no slight task. What would be requisite for the purpose, here designed, would probably be equivalent to one full year of work by four observers. The degree of accuracy really attainable in the use of the meridian circle should not be sensibly inferior to that for a vertical circle. The distinct advantage of the latter is in the variation of method which it offers. For the present we should advise the employment of a meridian circle for this research, unless the proposed Southern Observatory should be established on a scale which would enable it to maintain for certain lines of observation the highest ideals.

Whatever the precise methods of observation may be, the instrument, or instruments, employed should be used for at least two years in the northern hemisphere in the determination of the positions of the principal fundamental stars visible there. There would result a peculiar gain in precision through the comparison of observations with the same instrument upon the same stars, made alternately in the two hemispheres, by which certain errors of the instrument and

a part of the uncertainties in our knowledge of astronomical refraction could be in a marked degree eliminated. The proof of this is already at hand in numerous comparisons of observations made contemporaneously with different instruments in the two hemispheres. From these it can easily be inferred what would be the probable gain if both sets of observations were to be made with the same instrument. We think that this plan presents a unique opportunity which ought not to be neglected.

If the meridian circle should be employed with a full corps of assistants the southern work here proposed could probably be accomplished within four or five years from the date of beginning. It would involve about 40,000 observations of about 6,000 stars and about 4,000 observations of stars north of —20° for the purpose of comparison and check. From four to seven observers and computers would be needed, with as many more routine computers, in order to keep the work going continuously and the computations up to date.

(2) *Complete Observation of Stars to the Ninth Magnitude.*

The exact determination of the positions of telescopic stars down to the ninth magnitude and south of —30° of declination is a work of high importance. The opinions of astronomers would differ as to the pressing nature of this work in comparison with the two succeeding works mentioned in this schedule. As will be seen from the correspondence transmitted herewith, there are opinions of great weight equally positive in favor of each of these undertakings. The plan for meridian observation of faint stars is mentioned second in order because of its intimate logical connection with the investigation already mentioned under (1).

The project to observe with meridian instruments the precise positions of all stars down to the ninth magnitude originated somewhat more than thirty five years ago, and was adopted as the peculiar function of the *Astronomische Gesellschaft*, which at that time assumed something of an international character. The importance of this project has been universally recognized. The original program for the northern hemisphere was completed under a cooperative arrangement. The extension of this work under the same auspices has been carried to —22° of declination, and the observatory at Cordoba has pushed the work on to —32° of declination. About one quarter of the sky remains to be considered. The com-

pletion of this one quarter would greatly enhance the value of what has been already accomplished.

The demand for this work may be stated under three principal heads:

1. The primary utility of this work would rest upon its immense importance in studies relating to the stellar system. The work mentioned already under (1) would enable us to study the structure and motions of the sidereal system so far as this is represented by stars to the seventh magnitude. The extension of this work by means of (2) would take us to stars of the ninth magnitude, for which the older records of observations contained observed positions of the greater part. We should then be able to extend our studies upon the sidereal problem to stars ten times as numerous and more than six times fainter than those of the seventh magnitude. The feasibility of reaping the full fruits of this undertaking through the present generation of astronomers is not so great as for the brighter stars, but that which would be demonstrably attainable now in this direction fully warrants the enterprise. Furthermore, the completion of this monumental work, so that the accurate position of every star from the north to the south pole, down to the ninth magnitude, would be known for epochs near the beginning of the century, would be an achievement upon which the entire civilized world could look with pride.

2. The positions of these stars determined in this work would possess very great value as reference points in all micrometric work upon faint stars, nebulæ, comets, and planets observed by means of extra-meridian telescopes. This alone was originally looked upon as fully warranting the labor of the entire enterprise.

3. This work would furnish the reference stars needed in the work of the Astrographic Chart, which has for its aim the determination of accurate positions for all stars down to the eleventh magnitude. This forms the third step in a series of investigations which seek to determine the accurate positions of stars at successive epochs in order that we may ultimately learn their motions. The accuracy of which this third step is capable depends upon the accuracy of its basis, which must be the meridian observation of telescopic stars. The only difficulty is that the accuracy of the observations hitherto made under the program of the *Astronomische Gesellschaft* is not regarded as sufficient for the purposes of the Astographic Chart, as Director Loewy has pointed out. He recommends as a substitute for (2) the special determination of the star positions required for

the Astrographic Chart. It will be found, however, that the so-called zone observation of telescopic stars may easily be brought to a very much higher grade of accuracy than that which has prevailed hitherto, without any material sacrifice of the rapidity with which they can be made. This can be effected in three ways:

(*a*) By the use of a superior instrument.
(*b*) By the use of a more accurate and more extensive standard catalogue, rendered possible under (1).
(*c*) Through a better organization in methods of observation.

Then with the addition of about one fourth the number of observations which would be needed under (2), without reference to any ulterior use, we shall be able to complete the *Astronomische Gesellschaft* zones and at the same time meet the requirements of the Astrographic Chart; and we strongly recommend that this program be adopted.

The necessity of this extended program is all the more pressing because of the extremely doubtful probability that the observations for the basis of the Astrographic Chart can be secured by means of existing agencies with the requisite completeness and accuracy.

The argument that the positions of all the stars down to the ninth magnitude (and fainter) in the southern hemisphere can hereafter be derived from the Astrographic Chart assumes that we may look for the completion of that undertaking within a few years. The probability that the completion of certain important sections of that work may be delayed for a longer time than would be desirable, however, seems to be warranted by an examination of the situation. On the other hand, it seems very desirable that the great undertaking entered upon thirty five years ago for the determination by meridian observation of the position of all stars down to the ninth magnitude in the whole heavens should be completed.

The proposed Southern Observatory has here an opportunity to carry out a work which would have enough of intrinsic interest in and for itself, but which would possess the great added advantage of serving as the fundamental basis of another work even greater in extent than itself. It would also much increase the value of similar work already accomplished in the northern hemisphere. Furthermore, it would scarcely be possible to carry out the work proposed next in order (3) in an economical, effective, and comprehensive way, as to stars in sensible motion, without the completion of (2).

For this work there should be a corps of five or six observers and

computers and a somewhat larger staff of mere routine computers. Under these arrangements it should be possible in a good climate to turn out at least 20,000 accurate star positions each year, and to make all the computations necessary to put them in catalogue form. Since not more than 200,000 observations would be required, the entire work should be completed within ten years from beginning—possibly in less time.

(3) *Measurement of Stellar Parallax.*

The determination of the distance of individual stars is one of the severest tasks in the science of observation. The results already obtained during a period of more than sixty years, but chiefly during the last fifteen, are not free from troublesome discordances that seem to encourage pessimistic views. Yet a review of the work which has been accomplished, together with that of the larger amount which is now in progress, affords great encouragement that we shall be able to determine average distances of classes of stars with a very satisfactory degree of precision. This is the important thing we need to know in our first studies of the structure of the sidereal system. The two great questions to be solved are:

(*a*) What is the relation of brightness to distance? Are the stars of the sixth magnitude as far removed on the average from those of the second as the relation of brightness alone would lead us to think? According to this relation, the sixth magnitude stars should be on the average rather more than six times as far removed from us as are the stars of the second magnitude. Is this really the fact? It is a question for measurement to decide.

(*b*) What relation to distance has apparent motion on the face of the sky? Are the stars which appear to move athwart the sky nine tenths of a second per year three times nearer us than the stars which move three tenths of a second per year? It seems very probable that this is approximately the case, and that apparent motion is a better criterion of distance than apparent brightness. Whether this conjecture is correct or not can be settled only by actual measurements of distances.

The decision upon these points is a fundamental necessity in the stellar problem, because it opens the way for a much more powerful and economical solution of questions in relation to distribution in distance through the discussion of meridian observations. That way is, to some extent, open now, but we need that certainty upon

the correctness of the fundamental principle (*b*) or of some modification of it which can only be had through direct measurement of the distances of a large number of stars.

That such measurements in large numbers are practicable is demonstrated by the successful work of Gill and Elkin at the Cape and of Elkin and Chase at New Haven with heliometers, by Kapteyn at Leiden and Flint at Madison (Wisconsin) with the transit, as well as by other observers.

Some of the available methods for measurement of parallax are:

By the use of heliometers.
By the use of meridian transits.
By photographic methods.
By micrometrical methods.

All these methods have been tried extensively; the first has apparently proved most accurate; the last, formerly employed almost exclusively, would probably be discarded now by common consent. Photographic methods have not hitherto proved entirely satisfactory perhaps, and yet it seems to be the almost unanimous opinion of those in the best position to judge that this method offers the greatest promise for efficient work on a large scale when properly used. The method of exposing the same plate at three successive phases of parallax, suggested by Kapteyn some years ago, is the one which seems to offer the greatest promise of economy in labor and precision in the result. From existing evidence it does not appear that the photographic method is likely to be very effective upon stars brighter than the fifth magnitude.

The heliometer would probably be better suited for parallax measures of bright stars. This method, though extremely precise, is slow and costly. It has been employed at the Cape of Good Hope in measurements upon a few of the far southern stars; but, so far as is known, no work of the kind is now going on in the southern hemisphere. The Cape heliometer is now devoted to planetary observations upon a new plan, a work very appropriate to the original purpose of the Royal Observatory at the Cape of Good Hope. Therefore, with the exception of the aid which might hereafter be rendered by the Cape heliometer, the entire field for the determination of stellar parallax in the southern hemisphere is open to the Carnegie Institution, should it desire to enter it.

This work could be undertaken with advantage on almost any scale. Without any idea as to what means might possibly be avail-

able, we are unable to suggest a definite program other than the restricted one which follows.

In the first line it seems desirable that an attempt be made to measure the parallaxes of stars known to be in sensible motion. As the limit of such motion for a comparatively restricted work, one might take 0.″2 or even 0.″1. In order to identify the stars having such motions, and to measure the motions themselves, the sources of information are now very scanty. It would be almost a necessity to carry through (2) of the program—the meridian observation of telescopic stars down to the ninth magnitude. If this were not done it would still be necessary to make accurate meridian observations of all stars observed for parallax. The number which would be at present available under this plan and south of —20° of declination would probably be considerably under 500, though others could doubtless be found without much difficulty. We would recommend the use of a photographic telescope of relatively long focal distance for this purpose. It should be of the highest optical perfection. It is quite possible that a three lens or four lens combination would be advisable, in order to get sharp, round images over an area of at least four square degrees if possible. We suggest a telescope of 18 inches aperture and about 30 feet focal length.

It would also be highly desirable to measure with the heliometer the parallax of many stars brighter than the sixth magnitude and south of —20° of declination. A seven inch heliometer similar to those in use at the Cape and at New Haven would be suitable for this purpose. The stars selected should be chiefly those distinguished for proper motion—several hundred in number. This would be in effect an extension of the parallactic survey which has been carried on so successfully at New Haven within recent years.

We think that the services of two observers on this work for a period of at least eight years would result in an extremely valuable contribution to the solution of the stellar problem.

For some years the project of a general *Parallax Durchmusterung* has been brought to the attention of astronomers. This contemplates nothing less than the determination of the approximate relative parallax of every star down to the ninth or tenth magnitude. The inquiry would mean something like this: If we take as the unit of distance the average star of the ninth magnitude, what are the relative distances of other classes of stars, and what individual stars are especially near the solar system? A program for this purpose is described in the letter which Professor Kapteyn has addressed

to the Committee in response to its request (Appendix B). This would be an immense undertaking, which would seem to demand the cooperation of several agencies before it could be properly undertaken for the entire sky. It would also be desirable to learn more of the practical capabilities of the method proposed before making any recommendations upon the subject.

The plates which would be taken in the course of the attempt to determine the relative parallaxes of stars known to have sensible motion would themselves offer an opportunity for preliminary tests of the method and of the value of the expected result. It would also be easy to provide this in a more systematic form for restricted areas of the sky by the exposure and development of plates especially for that purpose, provided the measurements and computations for these plates could be arranged under a cooperative plan of limited extent, such as that suggested in the letter of Professor Kapteyn (Appendix B).

In view of these considerations we are of the opinion that it would be advisable, in connection with the proposed Southern Observatory, to set up a photographic telescope of about 18 inches aperture and 30 feet focal length for parallax work on the southern stars, and that it would probably be found desirable to maintain it in constant operation for a period of eight years at least. The services of two skilled observers and a small staff of measurers and computers would be required.

(4) *Measurement of Radial Motions.*

In close connection with the three projects already mentioned is another for the measurement of velocities of stars in the line of sight—*i. e.*, radial velocity toward or away from the earth. A knowledge of these velocities is of the utmost importance, and since the accurate measurement of such velocities has become possible, a most valuable source of information for verifying and enlarging the conclusions to be drawn from the discussion of proper motions has been placed in the hands of astronomers. In fact, through an adaptation of the investigations for solar motion in connection with these measurements of radial velocity, it is possible to obtain valuable conclusions as to the distances of various classes of stars having sensible proper motions. When a very large number of such measurements upon stars distributed over the entire sky shall have been obtained, it will be possible to determine the velocity of the solar motion with great precision. It will even be

possible to obtain in this manner an absolutely independent check upon the direction of solar motion which, in a problem so important, will possess the highest philosophical value and will become a valuable test of fundamental hypotheses as to structure and motion in the sidereal system.

Whether the stars are distributed with approximate uniformity in volume, and whether the motions are at random in every conceivable direction, are questions which require for a definite decision the added information which can be obtained by the measurement of radial velocities of large numbers of stars extending, if possible, somewhat below those visible to the unassisted eye, in combination with discussions founded on proper motions which result from meridian observations. What we now most need is such measurements for stars not visible from the observatories of the northern hemisphere. Scarcely anything in this line has been accomplished for the southern hemisphere. Recently the Lick Observatory has dispatched an expedition to Chile with the Mills three foot reflector to make such measurements, and the maintenance of this expedition has been provided for by Mr. D. O. Mills for three years. It may be expected to produce results of the highest importance—of importance relatively several times as great as would attach to like efforts with the same instrument in the northern hemisphere.

Thus we may hope to have at our disposal within a few years the measured radial velocities of practically all the stars in the whole heavens that are brighter than the fifth magnitude. This will be an extremely valuable result; but it would be made far more valuable if such measurements could be secured for a greatly increased number of stars over a greater range of magnitude.

Are the velocities of the more distant stars the same on an average as those of the nearer stars? Are the peculiar motions of the stars, after abstraction of parallactic motions, the same in the directions to and from the observer at different distances from the Milky Way? In order to answer these and similar questions with sufficient weight of evidence, the objects for which velocities in the line of sight have been measured should number 2,000, if possible; and in order to accelerate the rate at which results can be reached, we need telescopes of the largest possible light-grasp. There is no apparent obstacle, except cost, in the way of employing a telescope with a five foot mirror for this investigation.

That an increase in the resources for the measurement of radial velocities of stars in the far southern skies is desirable, appears from

the fact that, while there is permanently located in the southern hemisphere only one telescope which is used for this purpose, and provision made for the use of another during three years, there are employed in this service in the northern hemisphere at least six large telescopes, each more powerful than any telescope installed at a permanent observatory in the southern hemisphere. The disparity in resources here presented is marked, and the call for a remedy seems to be imperative.

It would therefore seem to be very desirable that the Carnegie Institution should enter this field and provide for use in the southern hemisphere the most powerful reflecting telescope that would be sanctioned by experience and the dictates of common prudence. The use of this in the measurement of radial velocities of southern stars should be provided for during six years at least. There would be needed a staff of two skilled observers and a half dozen measurers and computers.

The additional argument in favor of the provision of a large reflector to be used in the southern hemisphere will be found later, under (7), in this enumeration of proposed works.

The observations specified under the four preceding heads are closely related to each other, and logically they are branches of a single enterprise—an endeavor to make a strong forward movement in the solution of the sidereal problem. We consider it extremely desirable that provision for all the works enumerated by us should be made in the proposed Southern Observatory, but in the event of necessary curtailment which does not extend to the entire program, it is to be hoped that such curtailment may not apply to either of the four projects thus far mentioned.

(5) *Observations of Double Stars.—A Large Refractor.*

The measurement of double stars has been in active progress for more than a century. For the past seventy years work in this line has absorbed a very large proportion of the energies devoted to astronomical investigation. Certainly the class of facts developed by these investigations have remarkable interest, and they have exerted a deep influence upon the thoughts of man as to his place in nature. That the law of gravitation apparently extends throughout the universe; that suns revolve about suns; that the orbits are usually elliptical, like those of the periodic comets; that many of these bodies are larger and more splendid than our sun—these and

numerous other facts seem to lend a special significance to this branch of astronomy. Except incidentally, this work is not very closely associated with the stellar problem, and the immediate advantage of a greater extension of work in this line in the southern hemisphere is not so apparent; yet it should be remembered that no really large telescope has yet been applied for any great length of time to the measurement of double stars in the southern hemisphere, and while we put this project after the four already mentioned, we entertain no doubt of its desirable character.

It would seem very desirable that a telescope of about 27 inches aperture should be provided for this work. There should be a regular survey of the entire southern sky for the discovery of new double stars, to complete similar surveys carried out at the Lick Observatory and elsewhere.

Double-star work on the southern sky has come practically to a standstill, while it is still going on industriously at several observatories in the northern hemisphere. That a large telescope in the southern hemisphere should be devoted to double-star measurements for a period of about eight years seems to be evident.

One observer and one assistant would be required.

If a large refracting telescope should be provided for the proposed Southern Observatory it is very likely that certain micrometrical and other studies, apart from double-star observations, would be worth while, perhaps calling for the detail of another observer. This comparatively small expenditure would doubtless be well compensated in the increased utility of a costly telescope justified for another purpose.

Possible combination of (3) and (5).—In providing the instrumental means for carrying out (3) and (5) upon a restricted scale, it might be possible to combine the two telescopes required, so that one mounting and observing room would suffice. This would result in a very large saving in plant. The tubes of the two telescopes, each designed as perfectly for its peculiar purpose as if made for special instruments, could be attached to the same declination axis. Parallax plates should be exposed in the early evening and late morning hours, in any case. The result would be that the parallax telescope would usually be unused during the four or five hours around midnight, in order to avoid the use of the telescope at large hour angles east or west. During this period, if the parallax telescope were combined with the telescope for micrometrical observations the latter would be wholly free during four or five hours near midnight, the

hours which, on the whole, would be most suitable for the observer of double stars.

This arrangement is not suggested as ideal, but as one which might be adopted should the program for (5) be carried out and that for (3) be somewhat restricted. In that event we think the arrangement here suggested would be very much preferable, as it certainly would be very much more economical.

(6) *Variation of Latitude.*

The variation of the earth's axis of rotation is one of the newest and most interesting developments of astronomy. The facts relating to it have an intimate bearing upon the question of precision in meridian observations, in addition to the great interest which attaches to it as a physical phenomenon. Astronomy has not yet formed an adequate explanation of its origin. Quite recently Mr. Kimura, of the Japanese international latitude station, has called attention to a singular phenomenon developed by the international observations, which may be referred to several possible explanations, none of which appear quite satisfactory. In consequence of this, Dr. Chandler, editor of the Astronomical Journal, points out the necessity for establishing three observing stations in the southern hemisphere—one at the Cape of Good Hope, another at Sydney, and still another about 30 miles south of Santiago de Chile. This proposition has been heartily indorsed by several high authorities, including the Royal Astronomical Society. The operation of these three stations would tend to fix and define this anomaly in the observations beyond a doubt, and the result might be that the true cause of the phenomenon in question would be pointed out. Furthermore, the execution of this project would bring a most valuable contribution to the question of astronomical aberration.

We are strongly impressed with the importance of this work and are of opinion that, if the stations at the Cape of Good Hope and Sydney could be provided for by other agencies, the Carnegie Institution would do well to take the responsibility of the station in Chile. The annual expenditure would not be very great, and this project need not stand or fall with that for the Southern Observatory, although it has a logical connection with it.

The zenith telescope and observing shed required would not be costly, and two observers, without other assistants, would be able to take care of the observations and computations.

In addition to the undertakings we have already enumerated as offering a definite field of great usefulness for the proposed Southern Observatory, there are others which, in the judgment of many astronomers of high standing, deserve a prominent place in this enumeration. The following list briefly recapitulates some of these.

(7) *Astrophysical Researches.*

Should a large reflector be provided for the observatory in connection with (4) it could also be utilized with advantage for certain photographic and astrophysical researches of great importance to the advance of science. Among these are:

(*a*) Spectroscopic researches upon red and variable stars to supplement similar researches in the northern hemisphere.

(*b*) Photography of nebulæ in order to make the record of similar works complete for the entire sky.

(*c*) Spectroscopic examination of the nebulæ for the purpose of ascertaining their motions in the line of sight, as well as in the interest of inquiries into their physical nature.

These and similar works are named as supplementary to that mentioned under (4), not to be carried on to the detriment of the latter.

(8) *The Astrographic Chart.*

Allusion has already been made to the labors upon the "Carte du Ciel," or Astrographic Chart. Director Loewy, president of the Astrographic Congress, informs us that there is a vacancy in one of the zones; and he states that the services of the Carnegie Institution would be very acceptable in filling the vacant place. There is no question of the importance of this great project for securing a photographic representation of the entire sky at the present epoch. The work has already progressed so far that its ultimate success is now practically assured. It would be most unfortunate should the completion of one or two sections be delayed far beyond the completion of all the others, thus destroying the unity of epoch that is so desirable in all such works. This work requires the use of a thirteen inch photographic telescope of a special type. This instrument would not be very costly. The services of two observers and of a small corps of measurers and computers would be required during about eight years. We think that the feasibility of undertaking

this work ought to be taken into serious consideration by the Institution, should it be ascertained that there is no reasonable prospect of provision for it elsewhere.

(9) *Photometric Observations.*

Professor Seeliger urges the importance of precise photometry on the southern stars in the interest of problems relating to the structure of the sidereal system, and to supplement similar work carried on in the northern hemisphere. All the arguments which have been presented in relation to the first four numbers of this program apply to this. Statistical methods of investigation in this field have already led to significant and interesting conclusions, and we cannot doubt the power of this method, which has already proved such a valuable guide in affording reliable clues to the structure of the sidereal system. We think that Professor Seeliger's suggestion is worthy of further inquiry and consideration.

(10) *Researches of Minor Scope.*

A number of researches of minor scope have been suggested by our correspondents, all of which are of importance. For some if not all of them it seems desirable that interest should be excited at existing observatories in the southern hemisphere to take up these works and carry them to a successful conclusion. One of the good results which we should hope from the proposed Southern Observatory would be that it would serve to stimulate interest in astronomy throughout the populations of the southern hemisphere.

It will thus be seen that there is no lack of work of pressing importance to be done in the southern hemisphere. If an observing station should be established on a liberal scale for the execution of these works, it would still be a problem requiring wisdom and firmness to keep the program within practical limits and concentrated upon the furtherance of the great end desired.

THE QUESTION OF SITE.

We have given much attention to the question of a suitable site for the proposed Southern Observatory, but we are not yet prepared to make a definite recommendation.

For valuable information in regard to this question we are indebted to Mr. H. C. Russell, Government Astronomer at Sydney,

New South Wales; Mr. W. Ernest Cooke, Government Astronomer at Perth, West Australia; Dr. John M. Thome, Director of the observatory at Cordoba; Mr. Walter G. Davis, Director of the Argentine Meteorological Service, and Sir David Gill, Astronomer Royal at the Cape of Good Hope. We are greatly indebted to these gentlemen for the painstaking and valuable information with which they have favored us, for cordial offers of facilities, and for documents of interest which they have forwarded.

We have also devoted study to the meteorological and climatological reports in regard to the countries crossed by parallels of latitude suitable for the location of the observatory. In a preliminary way it appears to us that the most promising localities are in New South Wales, in the vicinity of Sydney; in South Africa near Bloemfontein, or on the Great Karoo plateau in Cape Colony, and near San Luis, in Argentina. San Luis appears to have a very clear sky and a salubrious climate. It is only 16 hours by rail from Buenos Aires. It is measurably free from the fearful "hondas" or stifling hot waves which characterize the Andean plateaus further west, and the skies are remarkably clear.

The latitude of Bloemfontein is rather smaller than is desirable, only 29° south, while —30° would seem to be almost the northerly limit admissible. Yet we have from Sir David Gill, who has inspected that locality in connection with the trigonometrical survey, the most favorable accounts of the wonderful transparency and steadiness of the atmosphere there, and of the remarkable number of clear nights, which is estimated at 300 annually, or about three times the number we experience upon the Atlantic seaboard. The elevation is about 4,000 feet above sea level. The mean annual temperature is, however, rather high.

Our early reports in regard to Australia were favorable, and we decided to procure a careful test of certain sites in the vicinity of Sydney. In April of the present year Professor W. J. Hussey, of the Lick Observatory, was appointed to make telescopic tests and other examinations with reference to observatory sites. He was engaged up to the end of July upon explorations in southern California, looking for a site for the Solar Observatory. Soon after the completion of this work he sailed for Sydney, on August 6, under instructions to test certain sites in the neighborhood of Sydney in relation to which we had formed favorable opinions, confirmed by the personal testimony of Mr. Russell. Mr. Hussey is provided with an excellent portable telescope of nine inch aperture, of which

the object glass was most obligingly loaned by The Alvan Clark and Sons Corporation of Cambridge, Mass., and of which the principal parts of the mounting were also loaned by the Lick Observatory, thus reducing the expense of the telescope to a nominal sum. With this telescope the character of the "seeing" is ascertained by systematic tests at all the stations visited. This can be compared with the excellent conditions prevailing on Mount Hamilton, where Mr. Hussey has had many years' experience.

Pending Mr. Hussey's examination and report, it scarcely seems worth while to enter into a detailed discussion in regard to sites at the present time. If the proposed Southern Observatory should be organized upon a scale sufficient to secure the execution of the greater part of the works enumerated in our program, the question of site would be very important indeed, and we have so regarded it. It is essential that the climate should be healthy for astronomers engaged upon tasks so strenuous; that there should be a large proportion of clear nights; that the air should be reasonably transparent and exceptionally steady, and that, so far as possible, the ordinary comforts of civilization should be found in the environment of the observatory. In the search for a stimulating and healthful climate, and also in the interest of the meridian observations, it would be desirable to choose a latitude of 40° south or more.

But in the southern hemisphere, in latitudes south of —35° or —40°, the amount of cloudiness is apt to be very great, or the climate is otherwise unsuitable. In other respects, as in what was formerly called Patagonia, some of the localities in high southern latitudes cannot be regarded as available. The vicinity of Hobart Town, Tasmania, offers many advantages, especially in the healthfulness and uniformity of its climate, but with the disadvantage of rain falling on nearly half the days of the year. The amount of clear weather in New South Wales, though superior to that of our Atlantic seaboard, is not quite all that could be wished. The object of the proposed observatory is to secure valuable observations in large masses by the expenditure of great energy during a comparatively short term of years. This qualification is not the only one, however, and may possibly be regarded as compensated somewhat if a locality can be found where the climate is healthful and where efficient routine assistants can be recruited from the surrounding population.

The funds appropriated for the use of the Commission will suffice for what it has undertaken to accomplish; but if site explorations

in South Africa or Argentina are desired, a further small appropriation will be necessary. Later, through the operations of Mr. Hussey and the kindness of Sir David Gill, the Committee expects to be in possession of better knowledge as to what may be advisable in this direction.

Buildings.

The question of buildings and other constructions necessary for the proposed observatory is one which cannot be discussed in its minute details until something shall have been determined as to the site. Since the idea of this observatory is that of a temporary observing station, to be occupied, perhaps, not more than ten or twelve years, our ideas of the construction required would be largely controlled by that fact.

The necessity of providing for the equalization of the inner and outer temperature seems to prescribe for the observing rooms a form of construction which would not be very different whether the observatory were to be temporary or permanent. The essential principle is that the walls should consist of an iron or steel framework, with an outer covering of wood, in the form of louver work, and an inner covering of sheet metal, such as galvanized iron. The efficiency of this form of construction has been fully tested and seems to leave nothing further to be desired. For the drum to carry a large dome, this form of construction would probably be as economical as any other that could be accepted. Wood might be used for the framework were it not for the necessity of an even and solid construction for the tracks upon which the rolling mechanism of roofs and domes is supported. For the meridian instruments, sliding roofs should be provided. The great superiority of these over the old form of shutters is now fully demonstrated.

Since an important requirement for site is excellence of atmospheric conditions, it follows that the proposed observatory must be located at some distance from any large center of population. Consequently it would be practically unavoidable that provision should be made for housing the observing staff upon the observatory premises. This is an arrangement which is quite indispensable to the highest efficiency in any case. In any but an extremely exceptional climate there will be a large proportion of nights in which the probability of clear sky during the first half of the night will be doubtful. If observers live within easy access to the instruments, much clear sky will be utilized that will inevitably be lost otherwise.

Furthermore, after long duty at the instrument the observer is in need of rest, and in order to attain it should not be subjected to the hardship of a journey on foot for a mile or two in the small hours of the morning. For those observers who are obliged to begin duty at some time after midnight, it is practically indispensable that their residences should be in close proximity to the instruments they are to use. Aside from these obviously practical considerations, it has been found by experience that the plan of housing the staff upon the observatory premises effects a real economy in the quality and quantity of output in relation to the total expenditure.

We are of the opinion, therefore, that residences for the observing staff should be provided upon the site of the observatory. All that would be needed would be small cottages of simple construction, suitable to the climate. Equal simplicity ought to prevail in the construction of the office buildings required for administration, computing, library, and storage. Small work rooms would also be needed for the mechanical department.

Staff and Organization.

The question of organization for the proposed observatory is one which cannot be effectively discussed until something shall be known of the definite purposes of the Trustees in relation to its establishment. It goes without saying that no work whatever should be undertaken which cannot be put under the direction of assuredly competent and energetic astronomers interested in what they are to undertake. If the scheme should be inaugurated in its broadest scope, as we hope, it would be a mistake to begin the execution of the plans until the general control and direction can be arranged in a manner to command the confidence of the astronomical world, as well as that of the Trustees.

In some respects, however, the situation would be peculiar. In the ordinary case one of the most important functions of the directof of an observatory is the control which he exercises in the choice or work and methods. In the present case the director would be partly shorn of this privilege at the outset, since the very idea of the proposed observatory would be the performance of certain definite tasks.

Furthermore, in the selection of the staff for the execution of the full program it would be necessary to select, in addition to routine assistants, about twenty astronomers and assistant astronomers of proved capacity and experience in varying degrees. Other qualifications, such as health, energy, and capacity of adaptation to new

surroundings, would have to be considered in an unusual degree To recruit this staff all at once, for temporary service, from the existing forces of astronomy might prove to be a somewhat difficult task. In case this project should be adopted, therefore, the Institution should be prepared for a somewhat gradual organization of the observatory, extending possibly over four or five years, before all its departments should come into full action.

This delay, however, might prove unavoidable from another point of view. The necessary provision for instruments and for their proper installation on so large a scale is a matter which cannot suitably be disposed of all at once. In the light of previous experience it may be estimated that a period of three or four years, at least, would be necessary before the means for observation could be prepared in all its details.

The Southern Observatory as an Expedition.

An important part of the astronomical work of the southern hemisphere has been the result of special expeditions. There are the early expeditions of Halley and La Caille and the later ones of Sir John Herschel to the Cape of Good Hope; of Johnson to the island of St. Helena; of McClean to the Cape of Good Hope for astrophysical work, and of others. The expedition of Captain Gillis to Santiago de Chile in 1850–'53 resulted in several extensive series of astronomical observations. The establishment of the Argentine National Observatory originated in what was essentially an astronomical expedition of the most fruitful character under the conduct of Dr. B. A. Gould. The extremely valuable work of Stone at the Cape of Good Hope was also virtually that of an expedition for a particular purpose. Nearly all the other astronomers who have done highly valuable work in the southern hemisphere have been northern astronomers who went to southern stations for some special work.

The idea of a temporary observing station in the southern hemisphere will not, therefore, seem to be in any way strange. In one respect it embodies an extremely economical principle—the observatory would be maintained only for the accomplishment of works deemed highly important. There would be no chance for it to pass through stages of comparative inaction or to engage in work which is comparatively less pressing, or in undertakings equally well done elsewhere. These are the dangers that may threaten a permanent institution.

Furthermore, the duties of administration would be somewhat simplified. It would chiefly be necessary to provide the means for ascertaining whether the works adopted are pushed with the requisite energy and skill—whether the product is that which was stipulated.

Pushing this idea to an extreme, the work of the observatory would virtually consist of a series of expeditions having scarcely any connection one with another, except that of proximity at the scene of operations. As fast as the preparations for one of these lines of work should be complete, the expedition for that would be despatched upon its mission, with its own head and its own staff.

The larger the establishment, the more necessary it would be to provide a strong observatory organization.

All these questions can be more effectively studied after it becomes known upon what scale the enterprise can be carried out.

III. Observatory for Solar Research.

As the central body of the solar system, confining the planets in their orbits by the power of its attraction and supplying them with light and heat through its radiation, the sun possesses for us an interest greater than that of any other celestial body. From one point of view this interest may be considered to be of a most practical character, since the conditions of terrestrial life are determined exclusively by the solar radiation, so that any possible changes which this radiation may undergo are likely to be of consequence to life upon the earth. From another standpoint the study of the sun possesses a philosophical interest of the highest kind, for the sun is a star, comparable in all particulars with countless stars which lie beyond the boundaries of the solar system, but possessing the unique distinction, through its proximity to the earth, of being susceptible of detailed study and investigation. Thus in all reasoning on the physical constitution of the stars, especially in connection with the great problem of stellar evolution, we must start from the sun as a type object and elucidate stellar phenomena from an intimate acquaintance with solar phenomena. We have no foundation for the hope that any other star will ever appear larger than a microscopic point of light, even though the telescopes of the future may completely outrank the instruments of the present day; but through the provision of more adequate means of studying the sun

we may hope immeasurably to strengthen and deepen the foundations on which investigations of stellar phenomena are laid.

Conversely the only means of studying the origin and development of the sun and of determining what it will become in the future is afforded by the phenomena of stars and nebulæ, for we find in the heavens stars in all stages of growth, illustrating every step in the process of evolution by which the sun has been developed from a nebula. Solar research should thus begin with the nebulæ, proceed with a physical investigation of those celestial objects which represent the earlier stages of stellar growth, culminate in a study of the solar structure and radiation, and conclude with an examination of the red stars, one of which the sun will some day become. The study of the sun, with the inseparably connected question of stellar evolution, thus presents a single great problem, important alike to the philosopher, the astronomer, the physicist, the chemist, the geologist, and indeed to every one interested in the study of nature.

Purpose of a Solar Observatory.

After full and careful consideration of the recommendations of the Advisory Committee on Astronomy * and an extended examination of the various questions involved, we respectfully recommend the establishment by the Carnegie Institution of a Solar Observatory, so situated and equipped as to permit the accomplishment of three principal objects:

(1) To measure the intensity of the solar heat radiation, and to determine whether it varies from perfect constancy during at least one sun spot period of eleven years. In connection with this investigation, to measure the absorption of sunlight in its transmission through the atmosphere of the earth and that of the sun, and also the radiation of different portions of the sun's image, such as spots, faculæ, and prominences.

(2) To bring to bear upon the solution of solar problems various modern methods of research, principally of a spectroscopic nature, which have not hitherto been applied with adequate facilities. More specifically, to provide for the investigation of various solar phenomena with the spectroheliograph, the visual and photographic study, with powerful spectroscopes, of the spectra of the chromosphere, sun spots, and for other researches of a similar nature.

*Carnegie Institution Year Book No. 1, Appendix A, p. 96.

(3) To provide, through the construction of a large reflecting telescope, for the investigation of various problems of stellar evolution, intimately related to solar work, which existing instruments are inadequate to solve.

It will be seen that the investigations here proposed may be grouped in another way, viz., (1) those which relate to the sun's radiation, mainly with reference to its effect upon the earth; (2) those which relate to the solar constitution, with special reference to the sun as a typical star; and (3) those which relate to the evolution of stars like the sun from nebulæ.

There are many important reasons to recommend the establishment of a solar observatory by the Carnegie Institution. Up to about the year 1875 a large amount of information regarding the phenomena of the sun's surface had been collected, partly through the utilization of more and more powerful telescopes, and particularly through the recent application of the spectroscope. But since that time, for reasons not easily to be explained, comparatively few important advances in the study of these phenomena have been made; very little advantage has been taken of the great improvement in telescopes and in spectroscopes during the intervening quarter of a century. No other department of astrophysical research has been equally neglected, and consequently in none is there such an exceptional opportunity for great advances. Only one of the twenty two refracting telescopes of from 20 inches to 40 inches aperture is regularly used for work on the sun, and with but two or three exceptions the solar spectroscopes in use are little better than those of a quarter of a century ago. Though such spectroscopes are fairly well adapted for the statistical work in which they are employed, they are wholly incapable of dealing with phenomena easily within reach of such spectroscopes as are used in physical laboratories. Even in the few cases in which important advances in solar investigations have been made, partly through the invention and perfection of new instruments, the means available have generally been inadequate to bring out the full powers of new methods of research, and atmospheric disturbances have always most seriously hampered observation. What is needed is an observatory at some suitable mountain site, where atmospheric disturbances are reduced to a minimum; the development of special forms of telescopes, particularly adapted to solar work, and the complete utilization of the numerous improvements in spectroscopes and other instruments for physical research which have been developed in the

physical laboratory and require laboratory conditions for their successful use. No existing organization proposes to do this work under these conditions, and there is no prospect that it will be undertaken unless by the Carnegie Institution.

Advantages to be Gained Through Improved Atmospheric Conditions.

Up to the present time practically all observations of the sun have been made from the lower regions of the atmosphere. This surrounds the observer in a vast fluctuating mass, which reduces the brightness of the heavenly bodies by nearly one half, and transforms their images, which should be sharp and clearly defined, into boiling and confused objects, in which the delicate details of the originals are almost wholly concealed. At rare moments of comparative calm, glimpses may be had of structure of indescribable delicacy, but if partially revealed for a moment it is instantly swallowed up by disturbances in the atmosphere. It is as though the astronomer were forced to make his observations from the bottom of an ocean, whose constant storms are not confined to its surface, but penetrate the utmost depths, churning them into a seething mass, through which all external objects seem vague and ill defined. It is evident that such disturbances in our atmosphere must prevent not only a clear and perfect understanding of the solar structure, but in no less degree an accurate and reliable measure of the intensity of the solar radiation, which will seem to vary with the fluctuations in the atmospheric absorption.

A sharp distinction must here be drawn between two very different kinds of disturbances which the atmosphere produces.

(1) In the measurement of the sun's heat radiation, to determine whether it varies from year to year, the *absorption* of the atmosphere is the principal obstacle. No solar image is required, and local disturbances due to irregular refraction are of little consequence. The absorption may evidently be obviated in large part by making observations from the summit of a very high mountain, at a point well above the denser portion of the atmosphere where most of the absorption occurs.

(2) The detailed study of the various phenomena of the sun's surface, on the other hand, is impossible without a large and well-defined image, free from disturbances caused by local inequalities of temperature in our atmosphere. Currents of warm and cold air, especially if they are in the neighborhood of the instruments, are fatal to successful work.

The absorption of the atmosphere for heat and light radiations depends mainly upon the length of the air path which must be traversed by the rays. At an altitude of 15,000 feet the most fluctuating part of the atmosphere has been left below, and the rare atmosphere above is subject to comparatively little variation in its absorptive power. If, then, an observatory for the study of solar radiation should be established at some such height above the earth's surface, in a region where but little water vapor is present, the difficulties hitherto experienced in the measurement of the solar heat would in large measure disappear.

But it by no means follows that such a site would be suitable for that department of solar research which requires a perfectly defined image of the sun. As a matter of fact, the sharpness of definition experienced during the day on mountain tops is frequently much inferior to that which may be found at lower levels. At the summit of Pikes Peak (14,147 feet), for example, although the transparency of the atmosphere is very marked, the sun's image is usually not well defined. The same is true at Mount Ætna (9,650 feet), except in the early morning when the low sun has not yet greatly heated the mountain slopes. At the Lick Observatory, on Mount Hamilton (4,208 feet), the day conditions are better, and are probably similar to those which are found at the Yerkes Observatory (1,100 feet).

Until recently no mountain peak has been known on which the sun could be observed to advantage throughout the day. This is presumably due in large part to the fact that the mountain slopes, if not thoroughly covered with foliage, become greatly heated by the sun's rays, producing ascending columns of warm air, which rise toward the summit and mingle with the cooler currents brought by the wind. Under such circumstances bad definition would be inevitable. At Mount Lowe (5,650 feet), near Pasadena, California, in a region where the remarkable uniformity of temperature and pressure would lead one to expect good definition, the solar image is frequently disturbed by currents of warm air rising from the unprotected slopes. Separated from Mount Lowe only by the width of a single canyon is Mount Wilson (5,886 feet). This mountain is well covered to the very summit with foliage, and thus stands in marked contrast with many of the mountains in southern California.* In consequence of this fact, all of the advantages to be expected from the exceptional quality of the atmosphere are experienced, without the disadvantages due to warm air rising from the heated

*See Professor Hussey's report, Appendix A.

slopes of the mountain. It is not surprising, therefore, that the sun's image, as seen from the summit of Mount Wilson, is apparently better defined than at any other point hitherto tested with a telescope.

But for the successful prosecution of solar research another condition must be fulfilled. The phenomena on the sun's surface are constantly changing in form, not only from hour to hour, but from second to second in the violent eruptions which are numerous during the period of greatest solar activity. In order to study these changing phenomena intelligently, it is necessary that they be kept constantly under observation. In observatories subject to frequent clouds and storms the progress of such solar changes cannot be steadily watched. At some critical moment clouds frequently interpose to prevent further work. In an investigation of the solar rotation, for example, it is of great importance that the position of a sun spot or a facula be determined day after day without interruption. In actual practice many of the photographs made at existing observatories are rendered almost useless on account of the cloudy periods which separate them from other photographs. Mount Wilson has the unique advantage of combining extraordinary perfection of definition with such freedom from clouds as to permit continuous work for months at a time.

Summing up, we may therefore say that, even with existing methods of research, important advances in our knowledge of the sun could be attained by providing for observations (1) of the solar heat radiation from some high elevation, and (2) of the phenomena of the sun's surface from a site such as Mount Wilson. In the beginning the work should be divided between two sites, but it is probable that the higher station could be given up after the relation of the atmospheric absorption at the two stations should become known. During the same period it would also be necessary to provide for simultaneous observations from a third point many thousands of feet below the high station, to measure the absorptive effect of a known atmospheric layer, in order that the total atmospheric absorption may be determined and eliminated.

New Types of Reflecting Telescopes and Their Use in Conjunction with Laboratory Instruments.

The exceptional opportunity which exists at the present time for advancing our knowledge of the sun by no means depends solely, however, upon the possibility of eliminating a large part of the disturbances due to our atmosphere. Even greater possibilities for

advancement lie in the application of new instruments and methods. Of first importance is the development of the telescope, including: (1) its construction in the horizontal form, especially for work requiring a large solar image and the use of spectroscopes and other instruments from the physical laboratory; and (2) its construction as a large, short focus reflector, equatorially mounted in the *coudé* form, and particularly adapted for the photography of nebulæ, the investigation of stellar spectra, and the study of the heat radiation of the stars.

Astronomical telescopes are of two kinds—refractors and reflectors. The former consist essentially of a lens mounted at the upper end of a tube, which is pointed toward the object to be observed. The lens forms, at the lower end of the tube, an image of the object the size of which varies directly with the length of the tube. Reflecting telescopes, on the other hand, consist of a concave mirror, usually of silvered glass, supported at the lower end of a tube which is open at its upper end. The rays from the object fall upon the mirror, which reflects them back and forms an image at the upper end of the tube. By means of an additional mirror this image is reflected out at one side of the tube, where it may be observed. In both types of telescopes the tubes are pointed directly at the object under observation, and the apparent motion of the object through the heavens is counteracted by a uniform motion of the telescope, produced by clock work.

The development of reflecting telescopes during the first half of the nineteenth century culminated in the great instrument of Lord Rosse, erected in 1845. The crudeness of the mounting of this telescope, due to the lack of suitable engineering facilities, rendered it useless, except for such visual observations as could be made in the absence of a driving clock. Partly for this reason the immense advantages of mirrors over lenses were not discovered, and during the latter part of the century attention was concentrated in large measure on the development of refracting telescopes. These advanced rapidly in size, from the 10-inch telescopes of Fraunhofer at the beginning of the century to the 15-inch Harvard telescope (1847), the 36-inch Lick telescope (1888), and, finally (1897), the 40-inch telescope of the Yerkes Observatory.

(1) But as equatorial telescopes increase in size, it becomes more and more evident that a limit must be set to development in this direction. The driving clock of the Yerkes telescope must move a mass weighing twenty tons with such precision that the image of

the sun will remain fixed in the field of view for hours together. Attachments weighing as much as 700 pounds may be carried at the lower end of the tube, but it is out of the question, in spite of the great size and strength of this telescope, to carry the large spectroscopes of modern times. Through the work of Rowland, whose construction and use of concave and plane gratings has done more than any other one thing to revolutionize spectroscopy, the spectroscope of the physical laboratory has become an instrument of large proportions and of correspondingly great power. Such an instrument may be as much as 40 feet in length, and even if it could be attached to a telescope, the bending of the spectroscope, resulting from its constant changes of position, would render it impossible to obtain sharply defined photographs of spectra. The changes of temperature which occur from hour to hour in an open dome, subject to the fluctuations of the outer air, would also interfere with the use of such spectroscopes for photographic work, even if their rigidity were perfect. As a consequence, the solar spectroscopes in use today are in almost every case practically identical with the instruments of a quarter of a century ago. Moreover, many physical instruments of recent invention and of extraordinary power are so constituted that they cannot be attached to a moving instrument : they must stand absolutely at rest, protected from the most minute disturbances, on massive piers, in a constant temperature laboratory. It is evident, therefore, that a telescope for physical work, if it is to be suitable for use with such instruments, must be so constructed as to bring an image of the sun or of a star into a physical laboratory provided with all appliances necessary to protect the delicate instruments from vibrations, from relative flexure of their parts, or, sometimes, from temperature changes of even a few hundredths of a degree, and to meet other requirements demanded in the most refined research.

The lack of a suitable horizontal telescope made it necessary for Rowland to confine his spectroscopic observations to the light of the sun as a whole, though the use of his powerful spectroscope for a study of the solar details would have yielded results of the greatest importance.

Other requirements of solar work remain to be mentioned. It has been found that a solar image seven inches in diameter offers decided advantages over a two inch image. There can be little doubt that with such atmospheric conditions as exist on Mount Wilson solar images of two feet or possibly even three feet in diameter

could be advantageously employed at times. The use of such large solar images should immediately permit advances of importance to be made. But it would be wholly impossible to secure such images with a refracting telescope of the ordinary type, for the corresponding lengths of the telescope tube would be 215 feet or 322.5 feet, requiring mountings of enormous dimensions, not only excessively costly, but wholly beyond the possibilities of construction. For this reason it is evident that another type of telescope must be employed if such large focal images are to be used.

Fortunately it is possible to meet all the demands imposed by the above named conditions. It is only necessary to mount the telescope tube horizontally in a north and south direction and reflect the light of the sun into the tube by means of a plane mirror driven by clock-work. This mirror is the only moving part of the entire mechanism. Its small size, as compared with a moving tube hundreds of feet in length, not only renders the problem of following the object a simple one, but it eliminates at once the entire question of the enormous cost of the moving tube, the dome, and the great elevating floor which, with an equatorial telescope, would be necessary in order to permit the observer to reach the lower end of the tube in all its various positions.

Heliostats of various kinds have been used for many years, but until recently no serious attempt has been made to construct a large horizontal telescope. The instrument of this type built for the Paris exposition was never completed, and its design was such that the conditions demanded in solar work could not have been met. The recently completed Snow horizontal telescope of the Yerkes Observatory and the horizontal telescope of the Smithsonian Astrophysical Observatory represent the type of telescopes here recommended for solar research. The Smithsonian instrument was designed for bolometric work, and it has proved to be admirably adapted for this purpose. The Snow telescope was designed for solar investigations requiring a very sharply defined solar image, and special precautions were taken to secure this result. Such a telescope accomplishes, at very small expense, the purposes already named; it brings a fixed image of the sun into a laboratory, where it may be observed with large spectroscopes or other apparatus mounted on piers.

These instruments demonstrate the possibility of constructing a much larger telescope of the same type, designed especially for solar work and provided with large spectroscopes and spectroheliographs. To be successful such an instrument should be mounted at a consid-

erable height above the ground, at a site like Mount Wilson, where the atmospheric conditions during the day are exceptionally fine. Under such circumstances a sharp and well defined solar image, from two to three feet in diameter, could be obtained, and numerous researches now entirely out of reach could be undertaken with every reason to hope for success.

Dr. Elihu Thomson has suggested that any distortion of the mirrors by the sun's heat can be obviated by making them of fused quartz, since the coefficient of expansion of this substance is almost inappreciable. Since Dr. Thomson has already succeeded in making small mirrors in this way, we include an item to cover the expense of the necessary experiments, which Dr. Thomson has very kindly volunteered to superintend.

(2) In work on the sun, as already remarked, the most important requirement is a large solar image produced by a telescope of great focal length, but in order to trace out the successive stages in the development of stars like the sun, a telescope of a very different type is required. The remarkable opportunities for advance in astronomy which exist at the present time through the possibility of building a large reflecting telescope were outlined in the Year Book of the Carnegie Institution for 1902 (p. 141). The optical parts of the Snow horizontal telescope consist exclusively of mirrors, and it thus preserves the peculiar advantages of the reflecting telescope; but for the photography of faint nebulæ, and for many other similar researches of fundamental importance in the study of the sun's origin and development, this type of telescope is not well adapted. What is needed is a mirror of the largest possible diameter and of short focal length, provided with a heavy and well constructed equatorial mounting.

To give an idea of the immense advantages of an instrument of this kind it may be recalled that with a two foot reflector an exposure of 40 minutes suffices to photograph stars that are invisible in the largest refracting telescopes. With longer exposures millions of stars can be photographed with such a reflector, of whose existence the largest refractors could never give any indication. A 5-foot reflecting telescope would collect six times as much light as a 2-foot, and nearly three times as much as the largest reflector now in use, and would open up certain fields of investigation now entirely closed. It would furnish means of photographing the nebulæ which would probably be superior to those offered by any existing telescope. It would also permit the heat radiation of some of the brighter stars

to be measured, from which valuable conclusions could be drawn as to their physical nature, and it would furnish the essential means of studying stellar spectra on a larger scale than that afforded by existing instruments.

Up to the present time such powerful grating spectroscopes as those of Rowland have not been employed for the study of stellar spectra. As already stated, the spectroscopes are too large to be adapted to equatorial telescopes, and the feeble light of the stars would demand exposures far longer than can be given under present circumstances; but with a great reflecting telescope, so mounted as to produce the image of a star in a constant temperature laboratory, there should be no serious difficulty in photographing the spectra of the brightest stars with the most powerful grating spectroscopes. The exposures might have to be prolonged for several nights in succession, but it would only be necessary during this time to maintain the spectroscope rigidly mounted on fixed piers at a constant temperature. With such photographs of stellar spectra a large number of problems, of great importance in connection with questions of solar physics, could be solved. For example, it would be possible to determine beyond question whether, as is now believed by some investigators, the red stars have on their surface a great number of spots like those of the sun. If the presence of numerous sun spots on these stars could be proved it would follow that as the sun grows colder, and advances toward the condition of the red stars, the spots on its surface will multiply in number. Such a conclusion would have a very important bearing on the problem of the solar constitution. Many other similar questions could be answered, such as those which relate to the relative pressure in solar and stellar atmospheres, the changes in the solar spectrum which will result from decreasing temperature, etc.

General Nature of the Principal Problems of Solar Research.

It should be evident from what has been said that improved instruments and methods of research, employed under atmospheric conditions more favorable than those experienced at existing observatories, should render important advances possible. Let us now consider briefly some of the principal problems of a solar observatory.

The Constitution of the Sun.

The problem of the solar constitution, though repeatedly attacked on both observational and theoretical grounds, still remains unsolved.

In addition to the bolometer, referred to elsewhere, the two principal instruments employed for this work in conjunction with the telescope are the spectroscope and the spectroheliograph. The former permits the investigation of the nature of the chemical elements in the sun, their physical condition, and their motions in the direction of the earth, while photographs taken with the latter show the distribution of the various elements in the solar atmosphere and on the sun's disk. The two instruments supplement each other most effectively, and can be used to great advantage in connection with the bolometer and other physical apparatus.

The sun closely resembles the earth in chemical composition, but differs from it in every other particular. Intense heat, indefinitely greater than that of a Bessemer converter, maintains its substance in a state of vapor. The visible surface of the sun marks the limit where the metallic vapors of the interior, coming into contact with the cold of space, condense into luminous clouds. This surface presents a granular appearance, since the bright clouds form the upper extremities of columns of vapor ascending from the interior, separated by spaces filled with cooler and less luminous vapors. Above the visible clouds columns of uncondensed vapors continue to ascend. Hydrogen, helium, and calcium rise to heights of several thousand miles, and at certain points project from the nearly continuous sea of flame (the chromosphere) in the form of great gaseous prominences, ranging in altitude from 15,000 to 300,000 miles. At times of greatest solar activity violent eruptions frequently occur, producing prominences which sometimes rise to a height of nearly 300,000 miles in less than half an hour.

Formerly the flames of the chromosphere and prominences were visible only at the sun's circumference, when at times of total eclipse the dark body of the moon intervened to cut off the overpowering illumination of the earth's atmosphere. In 1868 it was found that they could be observed in full sunlight with the spectroscope, and in 1892 they were first successfully photographed with the spectroheliograph. This instrument also permits the flames to be photographed in projection against the sun's disk, thus rendering possible the investigation of a great variety of new and remarkable phenomena. The invisible vapors of calcium, and recently, through the application of the Rumford spectroheliograph of the Yerkes Observatory, those of hydrogen, iron, magnesium, or any other substance present in the chromosphere, can be photographed at will, and their forms, distribution, and motions investigated.

The peculiar differences in the behavior of the various elements and the characteristic parts they play in solar storms suggest questions which are quite as important to the chemist or the physicist as to the astronomer. The high temperature and the enormous masses of material involved in these solar phenomena far surpass the possibilities of laboratory experiments. If the elements can be broken up into simpler forms by intense heat, as the tendency of modern research seems to indicate, the best chance of detecting evidence of such dissociation would appear to be in the sun. If calcium, for example, can be separated by heat into several constituents, these can be photographed with the spectroheliograph in solar storms, where their difference in behavior should betray their separate existence. Results already obtained at the Yerkes Observatory point to interesting possibilities in this direction; but, in order to deal with the problem successfully, apparatus much more powerful than that now employed must be available for use under superior atmospheric conditions.

Problems without number relating to the solar constitution are ready for solution and demand only a carefully planned attack under suitable conditions. The peculiarities of the solar rotation, rapid at the equator and decreasing toward the poles, have been but little studied. The distribution of the elements in the lower chromosphere and the explanation of the absorption which produces the dark lines of the solar spectrum have been subjects of dispute for many years. This problem is now studied only at eclipses, but a large solar image, observed on any clear day with a powerful spectroscope in a good atmosphere, would permit important advances to be made. The true cause of the darkness of sun spots is not yet understood, and it is even uncertain whether they are cavities or elevated regions. Their minute structure and the remarkable phenomena of their chemical composition will afford indefinite opportunity for research.

All of these questions and many others are closely dependent upon the sun spot period. As the spots increase in number and activity all other solar phenomena vary in sympathy with them, increasing toward the period of greatest intensity and then fading away toward the time of calm. The investigation should therefore extend over a term of at least eleven years, and preferably until after the maximum which occurs about 1916. The advisability of continuing the work longer may be left to be determined in the light of the results obtained.

The Heat Radiation of the Sun.

The advantages of studying the heat radiation of the sun at high altitudes are well illustrated by the results obtained by Langley from the summit of Mount Whitney in 1881. The dryness and purity of the air at this elevation in the Sierra Nevada are perhaps unsurpassed at any other mountain station. Over one half of the atmosphere lies below the summit of the peak, and this comprises the denser and more variable strata, which interfere most with determinations of solar radiation. No sooner had Langley's delicate apparatus been set at work than a new class of solar rays previously unknown was discovered. Previous estimates of the absorptive effect of the lower regions of the atmosphere were found to be far below the truth, and the value of the solar constant, which measures the amount of heat received by the earth from the sun, was immediately increased by about one half. The new radiations which were found to possess so great importance cannot pass through glass, and prisms of rock salt must be employed in studying them.

The Mount Whitney observations extended over only a few weeks, and were made with apparatus which, from his present point of view, Professor Langley would consider extremely imperfect. In spite of the unfavorable conditions that prevail at Washington, Langley has continued his investigations at the Smithsonian Institution and developed his methods to an extraordinary degree of perfection. Slow and inaccurate observations with insensitive instruments have given place to automatic records of the highest precision, secured by the aid of photography with instruments so sensitive that differences of temperature of less than one ten-millionth of a degree centigrade can be detected.

The problems which should be attacked from the summit of Mount Whitney, or some equally good station, with the refined instruments now available, cover a wide range. The most important single question concerns the intensity of the solar radiation. Is this constant, or does it vary during that well defined period of about eleven years in which solar phenomena are known to pass from a state of comparative calm to one of violent activity, and then again to subside to their former condition? At times of sun spot minimum the sun's surface for months together is wholly devoid of spots. Faculæ and prominences are few and inconspicuous, and the spectroscope shows little, if any, evidence of disturbances of any kind. Gradually, however, spots begin to appear, and then rapidly multiply in number.

The sea of flame which surrounds the sun increases in brilliancy and activity. As the spots continue to increase in number and area, eruptive phenomena on a tremendous scale become more and more frequent. At the time of maximum activity the violence of these disturbances and the rapidity with which flames hundreds of thousands of miles in height form and disappear surpass all comprehension. In view of these facts it is not surprising that the question has been raised whether the total radiation of the sun does not undergo variations corresponding in some measure with these variations in the violence of the phenomena visible on its surface.

The solution of this problem, as has already been pointed out, requires that observations be made under conditions not now available. With instruments of the modern type installed at a great altitude and with similar instruments at a second station some thousands of feet below, the principal conditions needed for the solution of this question would be provided; for it is necessary not only to measure the intensity of the sun's heat from an elevation so great as to eliminate most of the obstacles interposed by the denser and more fluctuating portions of the earth's atmosphere, but also to arrive at a more thorough understanding of the absorption which solar rays undergo in passing through our atmosphere, and particularly to determine the difference in the quality and degree of this absorption at different levels. The study of the earth's atmosphere is of great importance in connection with this research. Indeed, it is not improbable that after the completion of a thorough investigation, carried on simultaneously at the upper and lower stations and extending over a sufficient period of time, it would be possible to accomplish all of the purposes of a solar observatory at a lower station.

When observed in the telescope the sun's disk is found to be much more brilliant at the center than near the circumference The difference is so great that it was detected in the earliest observations of the sun, made with the imperfect instruments of the time of Galileo. This difference is due to an absorbing atmosphere which completely surrounds the sun, and reduces the intensity of the light and heat radiated outward through it. In a study of this absorption made by Vogel in 1877 it was found that at the edge of the sun's disk only about 13 per cent of the violet rays escape. The percentage of transmitted light increases progressively for the blue, green, and yellow rays, until it amounts to 30 per cent for the red. From these results it has been concluded that if the absorbing atmosphere were

removed the intensity of the sun's light would increase by as much as 1½ times for the red rays to 2½ times for the violet rays. Under such circumstances the color of the sun would appear blue. Later results indicate that the removal of the sun's atmosphere would increase its radiation of heat about 1.7 times. It is evident that if there were any considerable variation in the absorptive power of the sun's atmosphere, due to such changes as might easily take place during the passage from minimum to maximum solar activity, the total radiation of solar heat might be very appreciably increased or diminished. Indeed, such a variation in the solar absorption is considered by Halm, in his recent memoir on the solar constitution, to be the cause of the eleven year sun spot period. It is therefore evident that the plan of observations should include an investigation of the heat radiation of various parts of the sun's disk, carried on during a term of years.

Evolution of the Sun and Stars.

We have already called attention to the fact that while telescopic observations may give us an intimate acquaintance with the sun's surface phenomena, from which conclusions may be drawn as to its physical constitution, they can give us no direct information as to the past or the future of the sun. We must seek such information in the stars and nebulæ. Keeler's work with the Crossley reflector of the Lick Observatory brought out the extraordinary fact that there are in the heavens at least 100,000 nebulæ, the majority of which are of spiral form. It is the belief of many astronomers that this spiral form is indicative of a process of development, and that the teachings of the nebular hypothesis must be modified so as to accord with this new point of view. Advantage should be taken of every possible increase in the scale of the photographs by which the forms of spiral nebulæ are recorded, in order that further data may be obtained that may be of service in future attempts to modify the nebular hypothesis. Photographs taken with all possible instrumental refinements might well be expected to show changes in the nebulæ after the lapse of a comparatively short period of time.

While we may thus seek in the nebulæ for evidences of the sun's origin, its early life and subsequent development may be traced in the stars; for, in spite of their enormous distance from the earth and the consequent impossibility of observing their surface phenomena as we do those of the sun, the spectroscope is competent to

give reliable information as to their physical condition and state of development. The secular changes of any star are so slow that thousands of years might be required to detect them. But it fortunately happens that all stages of growth are now represented. It is as though one were to pass through an oak forest and seek to learn of the development of the trees. During the period of his stay no apparent change would occur. But he would have before him trees of the same species in every stage of growth: the acorn, the sapling, the oak in its prime, and the dying tree, representing the last stage in the evolutional process. By observing these, the evolution of a single tree could be understood. Thus in the stars, while their changes are too slow to be observed, a similar opportunity exists of tracing their life history. Preceding the condition of the sun we find stars like many of those in the constellation Orion, which have advanced but little beyond the state of nebulæ. Next to these come white stars like Sirius, which are but slightly condensed, and represent what the sun must have been millions of years ago. From these it is possible to pass by gradual steps through the process of development which leads to the production of yellow stars like the sun. Here the process of decline has apparently set in, and the beginnings of the last stage are faintly visible. Finally come the orange and red stars, the spectra of which reveal phenomena of the greatest interest, such as the sun will exhibit after the lapse of many centuries.

Existing instruments have sufficed to develop the main lines in this process of evolution, but there are numberless questions which cannot be answered without the aid of much more powerful telescopes. A great reflector, such as would be most suitable for photographing the nebulæ, would also be much superior to any existing telescope for the spectroscopic investigations which furnish the data required in this research. Reference has already been made to the possible existence of great sun spots on the red stars; these and many similar questions of equal importance in connection with the constitution of the sun cannot be solved until such instrumental means have been provided.

Throughout all of this work, both on the sun and the stars, it would be necessary to have constant recourse to laboratory experiments in order to interpret the observed phenomena. A suitable equipment of physical apparatus for this purpose is therefore essential in connection with a solar observatory.

Plans and Estimate of Cost.

As already explained, a principal station and two temporary auxiliary stations will be needed for the proposed Solar Observatory. We recommend that the principal station (A) be established at some such point as Mount Wilson, where Professor Hussey's observations have shown that the conditions are excellent for work on the sun requiring very perfect definition of the image. For the study of the solar constant and the absorption of the earth's atmosphere two auxiliary stations (B and C) will be required for use during the summer months. Station B should occupy a site at the summit of a high mountain, while station C should be near the base of the mountain, some thousands of feet below. In view of the time and expense required, Professor Hussey's study of mountain sites did not include the very high mountains. We have obtained much valuable information regarding conditions on Mount Whitney and other mountains, but a further study of the matter would be required before the sites for stations B and C could be decided upon. While Mount Wilson would probably serve the purposes of the principal station, there should nevertheless be a further and more prolonged study of sites for this purpose, to be made while the equipment is under construction. Recent experience indicates a probability that stations B and C might be discontinued after observations have been made at these points for two or three seasons, since it is probable that all the work of the observatory could then be done at such a point as Mount Wilson.

Principal Station (A).

The summit of Mount Wilson is about 8 miles in an air line from Pasadena. At present it is reached from the base of the mountain by either one of two trails, suitable for pack animals, but not adapted for wagons. One of the first considerations, therefore, would be the problem of providing adequate transportation facilities. This might be done (1) by constructing a wagon road from the foot of the mountain to the top; (2) by constructing an electric railway from Pasadena to the summit of the mountain; (3) by extending to Mount Wilson the electric railway that now terminates on Mount Lowe. Owners of the property on the summit of Mount Wilson state very positively their conviction that one of the two latter projects would be carried out in case this site were selected. This would naturally be an important consideration in the final decision

upon the question of site. Estimates of the cost of construction are based on the assumption that such means of access will be available.

Plan of Work.

The general plan of work which we believe should be undertaken at this observatory would comprise the following classes of observations. This program is of course subject to such modifications as experience may suggest.

(1) Frequent measurement of the solar constant, together with studies on the absorption of the solar atmosphere and the radiation of different portions of the sun's image, such as spots, faculæ, and prominences. The principal instruments needed for this research are a 16-inch cœlostat and large spectro-bolometer for the solar constant work, and a 30-inch cœlostat, with concave mirror of about 200 feet focal length, for providing a solar image suitable for detailed radiation work.

(2) Systematic observations, with large spectroscopes and spectroheliographs, on such problems as the solar rotation, the structure and nature of sun spots, faculæ, etc., and other problems related to the solar constitution. For this work there will be required two 30-inch cœlostats, used in conjunction with objectives and mirrors ranging in focal length from 64 feet to 200 feet; two large plane grating spectroscopes, having focal lengths of about 21 feet and 42 feet respectively, provided with auxiliary apparatus for work with the spark and arc; a three-prism spectroheliograph of about 10 inches aperture, and a three-prism spectroheliograph of 8 inches aperture and about 33 feet focal length. In order to secure the best definition of the solar image, the cœlostats should be mounted at a considerable height above the ground, and electric fans should be provided for stirring the air by Langley's method.

(3) Astrophysical researches on stars and nebulæ with a large reflecting telescope, provided with a three-prism spectrograph, and also with a concave grating spectrograph mounted in a constant temperature laboratory, for use with the reflector arranged as an equatorial *coudé* in photographing stellar spectra with very high dispersion.

(4) Laboratory investigations, mainly of a spectroscopic nature, on problems arising in connection with the solar and stellar work. Much of the apparatus provided for this purpose should be mounted and used in connection with the solar spectroscopes.

Buildings.

The buildings for the observatory should be of the simplest construction, designed for results, rather than for appearances.

A. Spectroscopic and Bolometric Laboratory.—Experience has shown that in order to obtain good definition during the day the instruments should be mounted at the greatest practicable height above the ground. In the preliminary design for a separate spectroscopic laboratory, the cœlostat piers were accordingly carried up to a height of 40 feet. A sketch was also made for a separate bolometric laboratory. Subsequently, from motives of economy, a combination was made of these two laboratories, wherein provision is made for one 16-inch and two 30-inch cœlostats. The two large cœlostats would be used for work requiring an image of the sun—one of them for spectrobolometric observations, the other for work with the solar spectroscope and spectroheliograph. The 16-inch cœlostat would be employed for measurement of the solar constant. The spectroscopes and spectroheliographs are mounted on piers so high as to be above the level of the constant temperature house for bolometric work. The piers may be constructed either of steel, properly ballasted, or of granite, which is found in abundance near the summit of Mount Wilson. The laboratory itself is of wood, supported on steel construction, anchored to the rock. In the plans submitted provision is made for mirrors of 200 feet focal length, but it is probable that it would ultimately be desirable to extend the house 100 feet to the north, thus permitting the use of a mirror of 300 feet focal length. The construction of the building is such as to reduce to a minimum the heating of the walls and the consequent currents of warm air.

B. Reflector Dome.—For a reflecting telescope of 5-feet aperture, mounted in accordance with the general plan shown in the drawing,* a dome of 50 feet internal diameter is required. The walls of the tower are built of thin she et iron, the track for the dome being supported upon columns of steel construction. The sheet iron wall is covered on the outer surface with wooden louvers in order to prevent heating by the sun. South of the telescope pier is a constant temperature laboratory, with piers for the concave grating spectroscope.

C. Office Building.—This includes offices and computing rooms for all the members of the staff, together with shops for instrument construction, library, laboratories, photographic rooms, etc., suitably

* Not here reproduced.

equipped for work. This building might be constructed of wood or possibly of rough fragments of granite. In any event, provision should be made for storing the photographs and other records in a small fire proof room.

In view of the isolation of the observatory and the important purpose of developing new methods and apparatus in connection with its work, provision has been made in the estimates for a very complete equipment of the instrument shop. It is understood that this machinery would be purchased and immediately installed in a shop if the Carnegie Institution were to decide to establish the observatory. The tools would therefore be used from the outset in the construction of a large part of the equipment.

D. Dwellings for Members of Staff, etc.—Simple cottages and bachelors' quarters should be provided at the observatory site.

Other items of expense of a general nature would include a telephone line and a line for transmitting electric power to the summit of the mountain, sewer and water systems, etc.

Stations B and C, for Solar Observations at High Altitudes and Studies of Atmospheric Absorption.

No recommendation is made at present as to the site of these stations, although it is possible that Mount Whitney and Lone Pine would prove to be suitable. The estimates of expense are based on the assumption that sites similar to these would be selected.

The plan of work at these stations would include simultaneous observations for the measurement of the solar constant and the determination of the atmospheric absorption. The buildings of the two stations should be alike, except for the addition of a few rooms required at the lower station. They contain, in addition to shelter for the instruments, small dwelling rooms for the four men—two at each station—who would occupy them throughout the summer months. The instrumental equipment of the two stations would be similar, consisting of a 16-inch cœlostat and spectro-bolometer for the solar constant work, together with a small miscellaneous equipment of instruments required in connection with this investigation.

General items of expense in connection with stations B and C would include means of communication between the stations, simple devices to maintain the constancy of temperature needed for bolometric work, provision for water supply and fuel, improvement of trails, shelter for laborers and pack animals, etc.

Staff.

A director, in general charge of all work, with special duties in connection with solar spectroscopy.

At Principal Station (A).—

Bolometry: An observer in charge of all bolometric work, with special duties in connection with the spectro-bolometer; an assistant observer at the spectro-bolometer, and four computers.

Solar spectroscopy: Two assistant observers, to carry on, with the director, solar spectroscopic work; two computers at the beginning, with the probability that this number will have to be increased during periods of great solar activity.

Stellar spectroscopy: One associate and one assistant observer; one computer, and one night assistant, on duty with the large reflector.

In addition, there would be required a secretary-librarian, who would also have charge of the accounts and the purchasing of supplies; a stenographer, an instrument-maker, a skilled machinist, a carpenter, a janitor, and two laborers.

At Station B.—One observer, one assistant observer, and a man-of-all-work during the summer months.

At Station C.—A staff like that at Station B, with the addition of one instrument maker.

LEWIS BOSS, *Chairman.*
W. W. CAMPBELL.
GEORGE E. HALE.

OCTOBER 8, 1903.

APPENDIX A TO REPORT OF COMMITTEE ON OBSERVATORIES

REPORT BY W. J. HUSSEY ON CERTAIN POSSIBLE SITES FOR ASTRONOMICAL WORK IN CALIFORNIA AND ARIZONA

Prof. LEWIS BOSS,

Chairman Astronomical Committee, Carnegie Institution.

SIR: Acting under your instructions, I left San Jose for southern California on April 16, 1903, to examine into conditions for astronomical work in that section, especially with reference to research upon the sun. I was to bear in mind the following requirements: First of all, excellent day seeing; scarcely second to this, excellent night seeing; fitness of site for living conditions, accessibility, availability of power, electric or other, either from abundant water sources or from commercial distribution, etc.

For the purposes of this survey, a 9-inch achromatic objective, focus 108 inches, was kindly loaned to the Committee by the Alvan Clark and Sons Corporation. The Lick Observatory loaned a Warner and Swasey micrometer, a declination axis and slow motion by the same makers, a good centrifugal driving clock, and many smaller pieces of apparatus needed in this work. A prism-and-grating spectroscope was loaned by the Chabot Observatory, and an excellent helioscopic eyepiece was supplied by the Yerkes Observatory.

The mounting of the telescope was made in the Lick Observatory instrument shops from my designs. Its general features will be understood from the accompanying photographs.* It was expected that tests in out of the way places would be required, and on this account the mounting was made as light as was consistent with sufficient rigidity, and the parts were so arranged that they could be transported on pack animals over mountain trails. The tube is constructed of aluminum sheets one sixteenth of an inch thick, suitably strengthened by end and center castings, etc. Its weight, complete with lens, tail piece, micrometer, and counterweights, is only 88 pounds. The wooden polar axis is provided with steel trunnions which turn in roller bearings.

* Not here reproduced.

The object glass is of excellent quality, and the mounting met the requirements in a satisfactory manner.

The equipment had been shipped ahead and was awaiting me in Los Angeles. In accordance with your instructions, I at once arranged for its transfer to Echo mountain, where Professor Larkin very kindly put the resources of the Lowe Observatory at my disposal, in order that comparative tests might be made with the Lowe 16-inch telescope and my instrument.

President H. E. Huntington, of the Pacific Electric Railway Company, through his general manager, Mr. Epes Randolph, extended the courtesies of the Mount Lowe Railroad from Los Angeles during my stay, thus facilitating the work.

Mount Lowe, like its neighbors of the San Gabriel range, rises most abruptly from the Los Angeles plains. One approaches the mountain by a mesa which rises so gradually to the precipitous spurs that he scarcely notices he has left sea level behind. Then a cable incline lifts him suddenly to an altitude of 3,200 feet. At the head of the incline are the power house and other buildings of the electric railroad. Some abandoned chalets stand by the edge of the canyon, whose precipitous eastern walls send back the echoes that give the place its name. Properly, Echo mountain is but a spur of Mount Lowe, which is a vast pile of just such spurs, culminating in a round, rocky, desolate summit, at 5,650 feet above the sea.

The original intention, it is said, was to place the Lowe Observatory on the top of Mount Lowe, but the electric railroad was never completed to the summit, and this plan was not carried out. The present terminus of the road is at Alpine Tavern, 4½ miles from the top of the incline and at 1,100 feet greater elevation.

The Lowe Observatory is situated at Echo mountain, 3,400 feet above the sea, a short walk above the head of the incline, and by the electric road is within fifty minutes of Pasadena and an hour and twenty minutes of Los Angeles.

The mountain rises immediately back of the Observatory, on the north, to an angular altitude of perhaps ten or more degrees, so that there is not a clear horizon in this direction. Toward the northeast, across a canyon, another spur rises to a greater elevation, and here, too, the view is obstructed. In other directions the horizon is clear.

The water supply of the Observatory is derived from a spring in one of the canyons a short distance away. A reservoir has been

constructed just in front of the Observatory and a few feet below. The water from the spring is piped into this, but not into the Observatory, the elevation of the spring being insufficient for this purpose.

Judging by surface indications, Mount Lowe as a whole is not well supplied with water. Nevertheless, in its canyons, even well up toward the top, one may find clumps of bay and sycamore trees, and these are known to require a fairly constant supply of water. It is possible, therefore, that by judiciously tunneling in the vicinity of these trees water could be developed in places where none is now visible on the surface except immediately after rains.

It soon became apparent that the prevailing level of the fog in the Los Angeles region is higher than that for the corresponding season in the neighborhood of San Francisco. It also happened to be the season of maximum drift, namely, April and May. I was therefore detained at Echo mountain beyond the time limit first set for my stay, the fog being around or above me fully two thirds of the time.

For this and other reasons, the suggestion to make tests at two other stations on Mount Lowe was modified, and only one was chosen. This was Inspiration point, at about 4,500 feet altitude, and accessible from Alpine Tavern. Here fog did not trouble, and tests were soon completed, several runs being made by the electric road down to Lowe Observatory for comparative tests with the 16-inch. On the 13th of May the 9-inch was finally dismounted, and the equipment was packed and sent to Los Angeles for shipment to San Diego.

My stay at Mount Lowe had shown that an elevation of 2,500 feet would in this section be entirely insufficient to escape the prevailing height of either the fog or the dust. It had also impressed upon me the enormous advantage of quick and ready communication with prosperous communities, such as the electric road made possible with Pasadena and Los Angeles, and of the electric power which that brought to hand for mechanical work.

I have spoken of the abruptness of the San Gabriel range. The usual mountain masses, however lofty, have a low altitude compared with the measure of their bases. Perhaps an average proportion in the California coast ranges would be that a mountain with an altitude of 1 mile would run a base line 15 miles to reach the last of its foothills. Mount Lowe has no foothills, and falls to the mesa's edge in 2½ miles. Mount Wilson, adjacent on the east, higher and broader, holds all its southern spurs well within a sweep of 4 miles

from the summit. Their slopes are practically unscalable except by constructed trails. Where the steepness does not forbid the way, the chaparral everywhere disputes it. This brushwood growth is peculiarly characteristic of the semi-arid mountains of southern California. Mount Lowe wears this covering of chaparral, but it is nowhere luxuriant. The decomposing white granite is inhospitable soil for vegetable growth. The mountain is sterile and forbidding, except in a few of its canyons. The white granite sand, scarcely covered at all with accumulating humus, stares everywhere through the irregular lanes in the chaparral. In many places the faces of the slopes lie almost bare and glitter unpleasantly in the sun. There is little doubt that radiation from the exposed surfaces accounts for the fact that the day seeing both at Echo mountain and at the higher station at Inspiration point averaged lower than the night seeing, which was, on the whole, excellent.

The term mesa is used in the Southwest to denote the fringe of detritus washed down from the mountains and sloping away from their bases to the plains, or the mesa may be formed, as in the region about Flagstaff, Arizona, by lava flows from volcanic cones; but always, if one examines it as a whole, one finds a sloping upland contiguous on the upper side to mountains and on the lower side to plains or valleys. It is in this sense of contiguous sloping uplands that I use the word in this report.

My departure from Los Angeles for the south was somewhat delayed by the accidental side-tracking of a part of the freight in its shipment down from Echo mountain. In the meantime I made my first trip to Mount Wilson, going up the Sierra Madre trail on the east side, staying over night, and descending the west or Pasadena trail on the second afternoon. Some days previously I had the good fortune to make the acquaintance of Mr. T. P. Lukens, of the United States Forestry Commission. He has been of the greatest assistance to me in furnishing reliable information respecting the mountains of southern California and northern Arizona. He spoke enthusiastically of Mount Wilson and advised me to see it. The only access to this peak at present is by the trails just mentioned. These are only wide enough for pack animals, burros, or mules, and about four hours is required for the ascent or descent. I had been assured that Mount Wilson was very different from Mount Lowe, though from the valley it has the same grim outlines; but I was quite unprepared for the actual view of it. Instead of one barren rock, a succession of rolling knolls forms the summit. The canyons con-

tain spruce and the ridges are covered with chaparral—a growth of Ceanothus (buckthorn), scrub oak, and other evergreen bushes, so luxuriant, so dense, that passage through it is quite impossible without the aid of an ax. This appealed to me at once as an ideal covering, changeless the year around, for the checking of solar radiation.

On exploration, Mount Wilson seemed to have in addition these points of advantage: A water supply remarkable for abundance and nearness to the summit; a small peak adjacent to and above the source of this water, somewhat higher than Wilson's peak, which could be utilized for pressure in a water system; abundance of granite rock, both dark and light, some of it showing excellent cleavage, for building purposes.

Absence of wind, as reported by every one acquainted with the place, is evidenced here by the straightness and symmetry of the trees, one apparent exception being the Ponderosa pine, with a great flat crown as if bent under the pressure of storm and wind. Later, I learned from a paragraph in a report of the United States Forestry Commission that the peculiar shape of this tree is a matter of age and habit and not due to local conditions.

The live oak is everywhere, both as tree and shrub, and in the canyons maple, alder, sycamore, bay, and cottonwood are found. Of the conifers, the big-coned spruce most abounds, and it is to this and the chaparral that the mountain owes its unbroken slopes of green.

In one thing only does Mount Lowe have an advantage, and that is in the possession of an electric road. As to electric power for mechanical and other uses, a line 4 miles in length from Wilson peak would connect with existing systems. An electric railroad could be constructed from Pasadena to the summit of Mount Wilson quite as easily as to the summit of Mount Lowe; or, as an alternative, it would be possible to extend the present railway from Alpine Tavern around Markham and San Gabriel peaks to the summit of Mount Wilson. It is estimated that such an extension would not need to be more than 10 miles in length.

On the evening of May 19 I reached San Diego and occupied the following day with business matters and acquiring information concerning conditions in the back country, especially as to the routes into the mountains. On the 21st I made an early start for the nearest of these, San Miguel, a peak 2,600 feet high, standing conspicuous and alone about 13 miles in an air line east of the town. The trip was made in a buckboard, as far as there was any

road, to the foot of the mountain, and then on horseback to the summit. Clouds lowered all the morning and hid the peak during the approach, but I had a clear and excellent view from the summit. I saw a country very different from the Los Angeles plains. It looks like an old land, and has the simple form and coloring that distinguish the arid regions everywhere. Sharp ridges rise ever more numerous back from the sea, but among the rolling dunes occasional peaks stand alone, boulder-strewn and desolate. San Miguel itself is a type of these—treeless from its top to its base.

The horses picked their way without difficulty through the scanty chaparral. Most of it stood just to their knees, though now and then a thicket of buckthorn rose to the riders' shoulders. From the summit to the base I looked in vain for any sign of springs or live water-courses, and there was almost no trace of animal life.

Here and there in the valleys below shone little reservoirs of water made by damming open streams. Now, at the close of the rainy season, none of these were full. I was told that the great Sweetwater dam, built at a cost of a quarter of a million dollars, has scarcely 10 feet of water behind it. There seems to be no bottom to this land, and in a season of ordinary rainfall most of the water sinks out of sight before it gets to these dams. The higher mountains, with their winter snows, are 40 miles away, and fluming is expensive business.

Riding back to town I read everywhere the story of the land of little rain. Just for a few miles at the foot of San Miguel a grassy mesa rolls irregularly down, but passes soon into the dunes of cactus and prickly pear which extend to the sea. In the little winding valleys orchards are dead or dying. Along the dry bed of the Sweetwater river pumps are trying to recover enough to save the trees of that district.

San Diego is an incorporated city of perhaps 18,000 people.

I went to Lakeside a few days later, and the San Miguel experience was in essentials repeated. Occasional orchards thriving, more abandoned, dying, or dead, according as owners have been able or not to hold out through the dry years and pay the high price of water in a drouth. At Lakeside I took the stage for Cuyamaca, a ride of 35½ miles. This region was chosen as the site of my station for the following reasons: My observations in San Diego had led me to believe that in this region, as at Echo mountain, any altitude below 4,000 feet would often be covered by fog at this season. On this account, as well as on account of the apparent lack of water within

1,500 feet of the summit, the establishing of a station on San Miguel seemed inadvisable. Of the higher mountains, Cuyamaca and Palomar seemed to promise most, but in San Diego I was able to get little definite information as to the latter. The impression prevailed that it was very inaccessible. Moreover, Cuyamaca was one of the mountains suggested by your Commission as worthy of exploration, and it has, besides, a daily mail stage from Lakeside to the Stonewall mine at Cuyamaca lake. The latter is at an elevation of about 4,500 feet, and there it seemed most expedient to make my first camp.

In San Diego I had received much valuable assistance from Mr. Ford A. Carpenter, local forecast official of the United States Weather Bureau. Through his interest I became acquainted with Mr. M. C. Healion, president of the San Diego Flume Company. This company controls the water from Cuyamaca lake, and has a house there. President Healion very kindly placed a room at my disposal, and his foreman in charge proved a very helpful assistant.

Cuyamaca is an Indian word, said to mean "cradle of the rains," and here is the heaviest rainfall in southern California. The lake is 2 miles long and less than 1 mile in width. The three Cuyamaca peaks rise about it—South peak to 6,500 feet, and Middle and North peaks to approximately 6,000 feet. Rattlesnake hill and a low circle of chaparral-covered hills shut off the view of the Colorado desert, whose first sentinel peak, just visible from the middle of the lake, is barely 6 miles distant in an air line, for Cuyamaca drops abruptly on the east. This nearness of the desert augured ill, and indeed the pull between the great oven and the sea kept the winds at work. The telescope was erected on the green slope at the base of Middle peak, near the dam and the west shore of the lake.

Grass is abundant here. Forests of oak rise back of the meadows, and above them the fir, the cedar, and the pine, in turn. The summits are forbidding, and their ascent with horses is impracticable unless road or trail has been cut. Up South peak such a road was made a few years ago by the United States Geological Survey, and that ascent is now easy. The view from this summit, supplemented by constant reference to the topographical map prepared by the United States Geological Survey, gave me an excellent idea of the entire surrounding country. To the north lay Palomar, beyond which rose the white cap of Old Baldy in the San Gabriel range, 110 miles away. Thence the San Bernardino and San Jacinto mountains lead toward Cuyamaca and the connecting ranges that

drop to the great desert on their eastern slopes. The Laguna mountains reach up from Mexico and thrust out a shoulder that just intercepts the view of the great Colorado floor stretching east to Yuma. Down to the west and south fall the lesser hills to the sand dunes and the sea, a country all creased and crumpled, arid and brown. Just below Cuyamaca lie Viejas and Elcajon, peaks whose altitudes and situations had appealed to me on the map as promising sites to consider. The field glasses bring them within a mile or two, and I find them, as I feared, exaggerated San Miguels; Elcajon more precipitous, Viejas more barren, equally treeless, and by all reports equally destitute of springs above their bases.

I learned that frosts may occur any month in the year at the lake; that the thermometer may rise above 100° in summer and fall below zero in winter; that the winds blow very nearly all the time, and that the lake dries up in summer.

The seeing at Cuyamaca proved to be what one might expect in a region of high winds and rapidly changing temperatures, and all that could be learned by inquiry and inspection failed to indicate anything more promising in this extreme southern end of the state, excepting Palomar only.

I had been told that one must return to San Diego, go by rail to Escondido, and thence by stage in order to reach Palomar. But I had looked across to it repeatedly from both North and South Cuyamaca peaks, and had concluded that one should be able to reach it on horseback from Cuyamaca, through Julian and either the Santa Ysabel or San Felipe valley into Warner's Ranch valley, which is at the southeastern base of Palomar. I set out, therefore, on the morning of the 6th of June. As I rounded the lake and turned northward I encountered light breezes from the desert. The peculiar subtraction of vital force effected by these desert winds is scarcely to be understood, but it is never to be denied by those who have felt it.

After a hard, all day's ride of about 40 miles via Santa Ysabel valley, through canyons, over hills, and across valleys, traversing roads at times remarkable for steepness, I reached Cook's ranch, on a shoulder of Palomar, late in the evening. I made excursions over the mountain on the following day, and the next day returned to Cuyamaca.

Nothing prepares one for the surprise of Palomar. There it stands, a hanging garden above the arid lands. Springs of water burst out of the hillsides and cross the roads in rivulets. The road is through forests that a king might covet—oak and cedar and stately

fir. A valley where the cattle stand knee deep in grass has on one side a line of hills as desolate as Nevada ; on the other side majestic slopes of pines.

Among the possible places for our purposes, a "bench" on Mr. Cook's ranch seemed especially attractive and caused me to consider most carefully the question whether it would be advisable to bring the equipment to Palomar. Your Committee has had in my reports descriptions of this place in some detail, and a balancing of the advantages and disadvantages of this site, the most promising in the San Diego section, as against Mount Wilson, the most promising in the Los Angeles region. This bench is situated at the southern edge of the broad, rolling, open space known as Dyche valley. Its contours adapt it admirably for all the requirements of an observatory site. Covered as it is with a good stand of maturing grain and fringed with magnificent oaks, its appearance is most inviting. Springs of water are found in nearby knolls above it, and a hundred feet or so below its edge an abundant stream breaks out that is said to run without change the year round. However, with the long California summer, no amount of irrigation could control the wide pasture lands and secure the changeless green surface of Mount Wilson. A few miles to the east, and less than 2,000 feet below, stretches Valle de San Jose, which heats like an oven and leads away toward the desert. The east winds in winter are said to be furious and not infrequent. The summer climate is almost uniformly pleasant, and the nights are always cool. The school session is a summer one, for snows in winter may block the roads and render them impassable for children. On the days that I was there the sea breeze was perceptible as early, certainly, as six in the morning, and it grew strong by noon. However, the bench I have described seemed especially sheltered by neighboring hills on the northwest and on the east, and its trees showed very little effect of wind. As soon as the sun was down, a decided chill was noticeable, and though the days had been warm, Mr. Cook proceeded, as a matter of course, to build a fire. The dews are heavy here, as at Cuyamaca. These facts were not favorable to one's expectation of the best seeing. The remarkable stillness, the steady temperature, and the evergreen covering of Mount Wilson could not be found on Palomar, though my judgment, from this cursory examination, would lead me to expect steadier atmospheric conditions here than at Cuyamaca.

There remains one other important factor against Palomar as a site for an observing station—its extreme isolation. Escondido is the nearest town of any pretensions, and that is 35 miles away. There is a road down the western side of the mountain, laid out to be a 10 per cent grade, but constructed steeper in places. A road is contemplated down the southeast end of the mountain, from Mendenhall valley into Valle de San Jose. This has been surveyed and, I was informed, the construction ordered. Whether railroads will soon come nearer the mountain's base than they are at present is entirely uncertain.

There are enormous disadvantages in the way of developing the country back of San Diego because of its remoteness, its surface configuration, and the insufficiency of water. It seems to me improbable that the power, the accessibility, the contact with civilization, and the resources of a city like Los Angeles, which Mount Wilson has at hand, will ever come within reach of Palomar. Moreover, its nearness to the desert and the absence of protecting mountains to shield it from the winds that play between the heated interior and the sea make it far from probable that we should have here the equable conditions required for the highest grade of astronomical work.

On returning to Cuyamaca I found that the equipment previously packed was already on the way to San Diego. On arriving there I received instructions to return to the Los Angeles region. The equipment was shipped at once to Pasadena and sent by pack train up the Sierra Madre trail to the summit of Wilson peak.

At the close of two weeks' tests at this station a week was spent at Flagstaff, Arizona, before returning north, where, by the courtesy of Mr. Percival Lowell, the records and instruments of the Lowell Observatory were freely put at my disposal to gain a knowledge of the conditions in that section.

The 9-inch telescope has been used at the following stations: Mount Hamilton, Echo mountain, and Inspiration point on Mount Lowe, Cuyamaca in San Diego county, and Mount Wilson.

The instrument was set up on Mount Hamilton as soon as completed for the purpose of testing the lens and mechanical parts of the mounting. Comparative observations were made with the 9-inch, 12-inch, and 36-inch telescopes. Some observations of the sun were made, but most of the tests consisted of observations of the stars, and particularly of close double stars.

It is well known that the maximum efficiency of a very large telescope can be secured only under excellent atmospheric conditions. It is also true that a large aperture is required for the most critical differentiation of the various qualities of seeing. It may very well happen that what may appear to be very good seeing with a 6-inch telescope will not prove so with an instrument of the largest dimensions. Moreover, different kinds of work vary in their requirements. For example, with a comparatively short photographic telescope no perceptible difference in the results will be found on nights of excellent definition and on those when the seeing is only fairly good. Again, for meridian-circle work the most essential condition is a steady image, and for difficult double-star work there is the additional requirement of fine definition.

Even though some classes of work may be carried on very successfully under circumstances which are not altogether favorable, it is nevertheless true that they would be more easy of accomplishment under excellent conditions; and it is also true that many important investigations require the highest obtainable efficiency of the most powerful telescopes for their successful prosecution. That site, therefore, which affords excellent seeing the most continuously, and the necessities, conveniences, and comforts of life the most abundantly, will be the best adapted for the needs of a great observatory.

For several years I have used the 36-inch and 12-inch telescopes of the Lick Observatory for double-star and other observational work, and on many occasions when working with one instrument I have gone to the other in order to make a comparison of the two. Whenever the seeing appears excellent with the large telescope it also appears excellent with the smaller one; but when it appears only fairly good with the 36-inch there is a tendency to rate it somewhat higher with the 12-inch. As soon as the tests began with the 9-inch it was further noted that there is a very perceptible difference between it and the 12-inch in the same direction. It was my especial object while the instrument was set up at Mount Hamilton to become acquainted with the characteristics of the image formed with the 9-inch under varying conditions, in order that I might properly interpret them and retain as far as possible 36-inch standards of excellence while making the tests in the various localities. To have proceeded in the opposite direction, by capping down the larger instruments to a 9-inch aperture would, in my opinion, have resulted in a lowering of the standard which it is

eminently desirable to maintain. It is manifestly impossible to give a numerical rating of the seeing which shall indicate its characteristics fully and without ambiguity. Nevertheless, it is convenient to use a number to express one's estimate of the effect of atmospheric conditions taken as a whole. The scale which I have used in this way embraces the numbers 1 to 5, inclusive.

The number 5 is used to denote seeing perfect in every way, a standard of excellence that seldom obtains; 4, excellent seeing with the excellent conditions lasting for considerable intervals; 3, good seeing, but with the images less sharply defined than indicated by 4, or having such conditions lasting for much shorter intervals; 2, poor seeing, images unsteady, large, or blurred, yet of such quality that good work of some kinds can be done, but not that requiring fine definition; 1, seeing so bad that good work with large instruments is out of the question.

At Lowe Observatory the 9-inch telescope was erected on April 23, between the observatory and the reservoir, and was dismounted on May 2. Observations of the sun were made during the forenoon, and tests of seeing at night—till midnight or later. On one forenoon only was the day seeing excellent. The first observations were made shortly after 6 a. m. At that time the sun's limb was sharply defined, and the granulations of the surface very clear and distinct. Several groups of sun spots were visible, and much detail could be clearly seen in both umbræ and penumbræ. At the time of the earliest observation the sun's image was remarkably free from passing heat waves, but this condition did not continue throughout the forenoon. The seeing gradually became worse, and by noon it was bad. It was the usual experience here to find the day seeing grow worse as the forenoon advanced, probably owing to radiation from the nearby and but scantily covered slopes of the mountain.

The night conditions at Lowe Observatory were on the average very much better than those prevailing during the day. Tests were made on all clear nights. Several were found to be excellent, others good, and only two bad, one of these being the night after leaving Inspiration point, when a violent storm was coming on.

In order to familiarize myself with 16-inch telescope conditions, I employed that instrument on parts of five nights in looking for new double stars. On each of these nights tests were made also with the 9-inch telescope. The following pairs, thought to be new, were found:

DM + 48°	1707	$8^h\ 49^m\ 16^s.3$	+ 48°35′.9	0.9	10 and 10, Comp. of $O\Sigma$196		
48	1716	8 53 16.6	+ 48 14 .5	0.3	8.5	8.8
50	1605	8 54 48.1	+ 50 28 .6	3.0	9.1...................	
51	1482	8 56 25.6	+ 51 12 .7	0.4	8.8	9.0
50	2174	15 19 25.3	+ 50 2 .2	3	8.2	12
50	2178	15 23 18.8	+ 50 49 .7	1	7.3	12
49	2408	15 31 40.2	+ 49 16 .9	0.4	8.2	8.6
51	2030	15 42 21.5	+ 51 14 .3	0.3	8.5	8.5
51	2077	16 15 57.9	+ 51 54 .7	3.0	8.9......	
51	2105	16 24 22.1	+ 51 54 .1	3	7.3	12
51	2106	16 25 1.2	+ 51 45 .4	4	6.2	13
51	2130	16 40 10.9	+ 51 48 .7	¼	7.5	7.5

I have not secured measures of these pairs. The first on the list is of special interest on account of its being the companion of a well-known third-magnitude double star, viz., 9 τ Ursæ Majoris, or $O\Sigma$196.

Alpine Tavern is situated among the trees near the head of a canyon. In its immediate vicinity there is no place that commands a clear horizon. A broad trail leads from it to Inspiration point, and narrow trails run up the mountain to the summit. No pack animals are kept at Alpine Tavern, and on this account it was not feasible to take the equipment to the summit of Mount Lowe. The only place within reach of the tavern that seemed suitable for the erection of the telescope was Inspiration point. The first observations were made here on May 7 and the last observations were made May 13, just before the telescope was dismounted.

At Inspiration point observations of the sun were made at intervals throughout the forenoon and the earlier portions of the afternoon, the first ones usually about 6.30 a. m.

It was found that the day seeing here is similar to that at the Lowe Observatory. Early in the morning the seeing was sometimes good, but it never had that excellent quality which was noted on one of the mornings at Echo mountain. During the forenoon the seeing always became worse, and in the afternoon it was never good. The mountain side to the east of this station was burned over only a few years ago, and it has not become covered with chaparral again. There is much surface exposed, and it is probable that the radiation from this is one of the causes of the seeing becoming worse as the day advanced.

Tests of the night seeing were also made, but they were not continued later than midnight. Some of the nights were excellent, others were fair, and none of them were very bad. It was possible

on three of the nights to take the car to Echo mountain and make additional tests with the 16-inch telescope.

The top of the Incline and the Lowe Observatory can be seen from Inspiration point. During my stay at the higher station I was constantly above the fogs which had hindered me so much at Echo mountain. They did not, during this period, rise to my elevation, 4,500 feet, but very often did cover Lowe Observatory all day long.

The telescope was erected at Cuyamaca on May 27, and taken down on the afternoon of June 5. Observations were made at intervals during the day, from shortly after six o'clock in the morning till the middle of the afternoon, and then again during the earlier portion of the nights. The results were not at all favorable. The seeing was nearly always poor, occasionally good, but never excellent.

The San Diego Flume Company has kept a record of the weather conditions at Cuyamaca dam almost continuously since 1888. I made some examination of this record, and from it derived considerable information respecting the weather conditions which prevail in this region.

The readings of the maximum and minimum thermometers show that the daily range of temperature is usually large, and that the weather sometimes becomes very cold in winter and very hot in summer. In winter the temperature occasionally falls several degrees below zero, and in summer it may rise above 100°. Thus, in June, 1899, there were five successive days with maximum temperatures of 104, 108, 110, 105, and 101. The minimum temperatures for the same days were 50, 46, 55, 55, and 54 degrees. The highest temperature noted in the record is 113 degrees.

The rainfall is heavier at Cuyamaca than at any other place in San Diego county. The heaviest snows also occur here upon the peaks, sometimes amounting to a depth of several feet. Even about the lake, in the little valley among the peaks, the snows are heavy. At one time last winter 30 inches lay upon the ground for six weeks, and ice formed on the lake to a depth of 8 inches. Here, as in other regions adjacent to the desert, water cannot always be counted upon from the melting of the snow. A warm dry wind may lick up the moisture and carry it away, leaving scarcely a trace.

The heaviest rains at Cuyamaca come in winter, but there are also local thunder showers in summer. During the last days of my stay in this region, I saw at a distance a number of these showers. It is said, however, that they are sometimes productive of very heavy rains. The prevalence of cloudiness in connection with these

showers and the disturbed atmospheric conditions indicated would without doubt prove an obstacle to the prosecution of solar work in this region.

The thermographic record obtained by the expedition at Cuyamaca extends over twelve days, from May 27 to June 8, inclusive. The temperature curve for this period is generally quite smooth and free from rapid alternations, but has, as a rule, a large daily range. This sometimes amounts to as much as 35°, passing in one instance from 46° at 6 a. m. to 81° at 4 p. m., and in another from 43° at 5 a. m. to 78° at 5 p. m. It was particularly noted that the only times of constant temperature were those when a strong sea breeze was blowing. Nevertheless, at such times, the seeing was invariably poor. The temperature curve at night was generally steep, often having a declining rate of from 3 to 4 degrees an hour. Only once was there a period of constant temperature at night. This lasted about four hours, with a variation of about 1°.

A thermograph was placed on North Cuyamaca peak on May 29, and allowed to remain there in a suitable shelter until June 8, thus giving a record for ten days. This record shows that more frequent small alternations of temperature were taking place on the summit than at the lower station, producing a less regular curve, especially during the day. The daily range in temperature on the summit was, however, much smaller. As a rule, it was less than 20°; the maximum was 29°. The night temperatures on the summit were quite satisfactory. On most of the nights there were intervals of several hours during which the temperature remained nearly constant or fell very slowly.

During the time I was at Cuyamaca the dews were heavy. This is a normal summer condition, due to the rapid fall of temperature at night.

The observations at Echo mountain and Inspiration point on Mount Lowe had shown that excellent night seeing prevails there. The day seeing there was also good at times, but it did not have that constant excellence which was deemed desirable.

Mount Wilson is only 3 miles east of Mount Lowe, and there was every reason to think that it has the same excellent night conditions that prevail on Mount Lowe. On the other hand, it appeared probable that it would have better day conditions, owing to the control of insolation and radiation by its dense covering of evergreen trees and chaparral. On going to Mount Wilson, therefore, I was instructed to devote the greater portion of the time to testing the see-

ing during the day. As will be seen from the details given below, the day conditions were found to be excellent; very much better than at any station tested in the south. Following is a record of tests made:

Thursday, June 18.—The erection of the telescope was completed this afternoon, but not in time to make any observations on the sun. Tests were made on the stars during the earlier portions of the night. The seeing was only fair, about 3.

Friday, June 19.—No tests were made during the forenoon. Tests made upon the sun during the afternoon showed the seeing to be good until 5.30 p. m., when observations were discontinued.

Saturday, June 20.—Tests were made on the sun at intervals from about 8.30 in the morning until nearly 6 in the afternoon. The seeing was fair in the morning, good in the early afternoon, and very good between 4 and 5 o'clock in the afternoon. Even at 6 p. m. it was nearly as good as the best day seeing we had at Cuyamaca. Very little wind.

Sunday, June 21.—Tests were made at frequent intervals throughout the day, beginning at 6.20 a. m. During the forenoon a pronounced sea breeze was blowing, the air had a chilly feeling, and the seeing was bad. In the afternoon the wind had decreased to a very light breeze, and the seeing was greatly improved. The sun spots have today been undergoing rapid changes. Some of these changes could be appreciated without difficulty or uncertainty after an interval of only a few minutes. From 3 to 4 o'clock the seeing was at its best. The granulations of the surface, the detail in the spots—both in umbræ and penumbræ—were then very clear and steady. The appearance at this time may best be described by saying that the effect as to clearness, sharpness, and steadiness was very like that of a print from a steel engraving.

At 4.30 the seeing was not so good as between 3 and 4 o'clock, but still the details in the umbræ and penumbræ of the spots, the filamentary character of the umbræ and the granulations of the sun's surface were very distinct, and the periods of steadiness lasted for considerable intervals. Between 5.30 and 5.45 the seeing was not so good as earlier in the afternoon. The image was somewhat less steady, and occasionally considerably blurred.

No observations were made at night. There was a light wind at Martin's camp during the evening, probably less than 10 miles an hour.

Monday, June 22.—The sun was examined from 6.30 to 7.15 a. m., and the seeing was found to be poor. A pretty strong west wind was blowing. During the forenoon the sky clouded and it remained cloudy until late in the afternoon. Unquestionably a storm condition prevailed during the day, and no doubt its effects lasted into the night.

From 8 to 10 p. m. the seeing was fair, about 3. The stellar images were small and distinct, but with considerable motion and more or less continual breaking of the diffraction pattern. A strong northeast wind, perhaps 20 miles an hour, was blowing during the night.

Tuesday, June 23.—From 3 a. m. to 4.15—*i. e.*, dawn—the stellar images were much better than in the earlier portion of the night. The central disc of the image was small, distinct, and sharply defined, and the diffraction pattern was much more satisfactory.

Very early in the morning the wind went down almost completely, and when the sun was first examined, from 7 to 7.20 a. m., the image was found to be excellent. The granulations of the surface and the structure of the spots were very clearly defined and almost wholly free from blurring.

By 10.30 a. m. the seeing had gone to pieces. While the image was occasionally clear and distinct, this was by no means the normal condition. On the contrary, it was during the greater part of the time more or less completely blurred.

Between 11 and 12.10 the seeing was considerably improved. There were intervals when the definition was good, but longer periods when it was poor, and occasionally the blurring was bad.

Between 2 and 3 o'clock the seeing was poor. It clouded up about the middle of the afternoon and remained cloudy until shortly after dark. It then became beautifully clear. During the night there was a strong wind from the northwest.

Wednesday, June 24.—Early in the morning, 6 to 7.30, the seeing was not very good, nor was it very bad. There was more or less constant haziness without pronounced blurring. Much detail could be seen, but not very satisfactorily. It did not have the steel plate effect of the finest definition. From 9.30 to 11.45 the seeing was poor. In the afternoon it was fair between 4 and 5 o'clock and somewhat worse during the next hour.

In the evening, from 8 to 10.30, the seeing was good—3 to 4. This was also the case during the latter part of the night—*i. e.*, from 3 to 4.15 a. m.

Thursday, June 25.—Between 6.30 and 7.15 a. m. the seeing was excellent in every way, the image very clearly defined and almost wholly free from blurring—steel plate effect.

Between 9.30 a. m. and 12 m. the image was less satisfactory than early in the morning. The limb of the sun was not so steady, and blurring was more frequent and more pronounced.

In the afternoon, between 4 and 5.45, the seeing was good, but by no means equal to that which we had early in the morning. Professors Campbell and Hale arrived at Wilson peak this afternoon. Professor Hale rated the afternoon seeing at 4.

Friday, June 26.—Early in the morning, from 6.15 to 7.15, the seeing was very good, but not equal to that of yesterday morning, nor to that of last Sunday afternoon. Between 9 and noon it was less satisfactory. It was then fair and was given a rating of 3.

During the middle portion of the day it was very warm. No observations were made in the afternoon until about 4 o'clock. The seeing was then excellent. Professor Hale rated it at 4. It continued good until nearly 6 o'clock.

In the early part of the night the seeing was good, but not excellent. The central discs of the star images were small and distinct, but the diffraction patterns were more or less in motion.

Saturday, June 27.—Early in the morning, between 6 and 7.15, the seeing was excellent. Later, during the afternoon, it was fair. In the latter part of the afternoon it was found to be excellent again.

Sunday, June 28.—Greater part of the day spent in exploring the mountain in company with Professors Hale and Campbell, and Messrs. Staats, Holmes, and Lukens, of Pasadena.

About 8 o'clock the seeing was found to be very good, and it was also good in the middle of the afternoon.

Monday, June 29.—Partially cloudy most of the day. No observations made during the forenoon. At 3.30 p. m. the seeing was not good.

Tuesday, June 30.—Seeing good in the morning and also in the afternoon. Most of the day spent in photographic work. Seeing good in the evening.

Wednesday, July 1.—No observations were made during the forenoon. The seeing was excellent between 4 and 5 o'clock in the afternoon. In the evening a wind arose and the seeing went to pieces.

Thursday, July 2.—At 9 a. m. the seeing was good. There was some blurring and a little disturbance at the sun's edge, but these

were not very pronounced. The seeing was rated at 3. Telescope was dismounted during the forenoon.

The summit of Mount Wilson consists of perhaps 200 acres of more or less rolling ground, intersected by ravines of moderate depth, sparsely covered with splendid pines, with open grassy spaces of considerable extent, affording ample and excellent sites for buildings, large or small, with sufficient room to meet the needs of a large community. The summit proper is situated near the southwest point of this area. Here, in the vicinity of the old building known as the Casino, is, in my opinion, the best site for observatory purposes.

To the southwest of the Casino the ground drops away rapidly and is covered as far as one can see down into the canyon with a dense growth of chaparral, giving an evergreen covering that effectively shades the ground at all times. The spurs to the southeast of the summit are also largely covered with a growth of chaparral and in places also with a larger growth of oak and pine. In this direction, however, the covering is not always so complete as toward the southwest. This is due to the more precipitous character of the mountain side in this direction, making it less easy for vegetation to gain and retain a foothold. Nevertheless the whole barren areas are of comparatively small extent, and this is especially so in the vicinity of the Casino. Moreover, the barren areas near the Casino are not generally solid rock, but slides of decomposed granite, and over these slides it would be possible with intelligent care to foster a growth of chaparral which would eventually more or less completely cover them.

In this connection it is well to hold in mind the abundant supply of water that is within reach at Strain's camp, only a third of a mile north of the Casino and less than 300 feet lower. Here are wells which at the present time are constantly overflowing. Even after a series of dry seasons it was found that the upper of these wells alone would by pumping yield a constant supply of not less than 25 gallons a minute. This would be sufficient for the scientific and domestic purposes of a large observatory, and also for irrigation purposes, should irrigation ever be found desirable. Considering their nearness to the summit and the difficulties that are ordinarily encountered in finding water near the tops of mountains in southern California, these wells must be regarded as little short of phenomenal. If Mount Wilson should be selected as the site for an observatory, it would be eminently desirable that the Observatory

should control at least one of these wells and its immediate watershed.

It is not known how difficult it would be to obtain a large supply of water at any other place near the summit of Mount Wilson. It is certain that wells or tunnels at many places would prove failures, as did the first one that was designed to furnish a supply of water for Martin's camp. Martin's camp is, in an air line, about half a mile south of the summit, and 700 or 800 feet lower. It is now supplied by a tunnel on the west side of the mountain at a distance of perhaps 500 feet in altitude below the summit. It is possible that the resources of this tunnel might be materially increased by extending it further into the mountain, or by running drifts parallel to the mountain side. This, however, is by no means certain. The result of further tunneling would depend almost entirely upon the character and arrangement of the strata encountered in making these extensions, and what these would be in a granite mountain densely covered with brush is not easily foretold.

As it stands today, the tunnel that supplies Martin's camp does not seem to afford a sufficient supply of water for a large community, such as might eventually grow up around an important observatory, and it certainly would not furnish the abundant supply that would enable water to be used extensively for irrigation, which would be necessary, for a time at least, if an attempt were made to clothe the barren slides with a covering of chaparral. The greater distance of the present tunnel below the summit as compared with the wells at Strain's camp is a forceful argument in favor of the latter.

An undulating ridge connects Mount Wilson with San Gabriel peak. Upon this ridge, at a distance of perhaps half a mile from the Casino, is a summit locally known as Alta, which rises about 60 feet higher than Mount Wilson itself. Alta is not of large dimensions. If an observatory were placed upon Mount Wilson it would be the most favorable site available for the storage of water. The pressure obtained from a reservoir placed here would not be very great, but would be enough to make it of extreme importance as a protection against fires and as a distributing center, etc.

While in southern California, from April 17 to July 19 (with the exception of the interval from July 8 to 16, when I was in Arizona), I watched the drift of ocean fog over the land in reference to its bearing upon the height at which an observatory would have to be placed in order to be above its prevailing higher levels.

In the Los Angeles region it soon became apparent that the height of the fog at this season varies greatly on different days. One morning it will be lying almost in contact with the ground and not advancing far from the sea. More frequently, however, the upper level has an elevation of from 3,000 to 4,000 feet, only occasionally exceeding the latter altitude.

At Echo mountain the upper surface of the fog was sometimes just below the Lowe Observatory. Whenever it was very near, the seeing seemed to be unfavorably affected. When it was a thousand feet or more below, no manifest influence was noted, either for good or bad.

The same characteristic was noted at Inspiration point. Here, also, when the fog rose in the canyons to an elevation nearly equal to that of my station, the seeing appeared to be unfavorably affected.

Mount Wilson is somewhat protected from the direct drift of the ocean fog by a spur of Mount Lowe, and during my stay at Mount Wilson the fog did not at any time approach very near the summit. At night currents of air were noticed descending the sides of the mountain, and no doubt there were ascending currents during the day. No ill effects from them were noticed.

In the San Diego region, near the sea, fog is, according to the United States Weather Bureau record, of daily occurrence during most of the year. In San Diego, in summer, it is generally cloudy at 5 a. m, and often so at 5 p. m. The middle portion of the day is usually clear, or partially so. The record indicates less fog in winter than in summer. The record does not contain any information respecting the altitude of the fogs. I was in San Diego county twenty-five days, from May 19 to June 13, at a season when fog is said to prevail to its greatest extent. On fifteen of these days my observations showed that the summit of San Miguel, whose height is 2,600 feet, was either below or in fog or cloud a portion or all of the day. On three days it was clear at the summit. On the remaining seven days I was not where the peak could be observed.

At Cuyamaca the ocean fog, blown in by strong westerly winds, reached my level on two occasions, and I several times saw it to the west, covering all the peaks in that direction lower than 3,500 feet.

The prevalence of high fogs in these regions makes it evident that an observatory should be placed at an elevation of more than 4,000 feet.

Many of the days and nights at Mount Wilson are nearly windless. Strong gales do sometimes blow ; some violent storms may be

expected every year, especially in winter; but they are not numerous and are to be regarded as exceptional.

Altogether, I spent forty six days on these mountains. In that time there was one severe storm, when the wind was high—probably as much as 60 miles an hour. In addition to this, there were four or five other nights when the wind velocity rose to 25 or 30 miles an hour. Otherwise, the days and nights were very quiet. From all that I could learn, it appears that this record is not more favorable than the average.

Mount Wilson and Mount Lowe are on the southern edge of the San Gabriel mountains, and near the middle of the range from east to west. Here the range has its greatest breadth but not its greatest height. To the northwest of Mount Wilson there is a succession of ridges and peaks that rise to elevations of from 5,000 to 6,000 feet. But it is to the north, northeast, and east that the highest peaks of the range are situated. In these directions, beyond the San Gabriel river, numerous ridges may be seen, some more than 7,000 feet high, with peaks that tower above them, culminating in San Antonio (Old Baldy), 10,080 feet above the sea. These higher ridges lie directly between Mount Wilson and the Mohave desert. By reason of their situation and height they no doubt greatly protect Mount Wilson from winds that would otherwise blow across this region between the desert and the sea.

These higher peaks and ridges are from 10 to 25 miles away. Deep, broad canyons lie between, winding in and out among the mountains until they reach the valley on the south. The advantages of this arrangement from an astronomical standpoint will be readily understood. Streams of cold air settling down from these higher peaks cannot flood Mount Wilson. This source of bad seeing, therefore, appears to be wholly absent. San Gabriel peak is somewhat higher than Mount Wilson, but there is a deep col between them. Flooding with cold air appears to be a fruitful cause of bad seeing at some observatories. Thus Mr. A. E. Douglass speaks of the condition at Arequipa, Peru:

> "The observatory is situated close to a river, down which on clear nights a swift stream of cold air descends. When this cold air reached the telescope, the seeing was immediately ruined. When this current was once established, no more good seeing could be expected for the remainder of the night." *

At Flagstaff also the temperature often falls suddenly in the latter

*American Met. Journal, vol. II, p. 395.

part of the night, and at such times the seeing is said to go to pieces.

The San Gabriel and San Bernardino mountains trend nearly east and west, and were it not for the depression between them at Cajon canyon, through which the Santa Fe Railroad passes, they would form a continuous range. The highest points in the San Bernardino mountains, viz., San Bernardino peak (10,630 feet) and San Gorgonio mountain (11,485 feet), are about 40 miles southeast of Cajon canyon. From these high peaks this range dips down toward Cajon canyon, which itself has an elevation of about 2,800 feet above the sea. The main portion of the Mohave desert lies north and northeast of the San Bernardino range. In the vicinity of Cajon pass it has an elevation of about 3,200 feet. Over the desert the air is subject to large and rapid variations of temperature; over the sea, only 70 miles away, it is not. The result of this is a movement of the air which follows the line of least resistance. This happens to be Cajon pass, noted in southern California as a very windy place. When the winds from the desert are strong they sweep violently through this pass southward toward the sea, in the direction of the Santa Ana mountains, and there the Santa Ana canyon affords a passage for them. It, too, is noted for its winds—so much so that in this section of the State any very strong wind is called a Santa Ana. An inspection of the map will show Mount Wilson far to one side of this usual track of strong winds. It is only when the pressure over the desert becomes so great that the volume of moving air cannot be passed through the dip in the range at Cajon canyon that the winds come across the mountains directly and sweep over Mount Wilson from the north.

Only a small portion of the Mohave desert lies directly north of the San Gabriel range. It sometimes happens that winds originating here sweep around the western end of the range through Soledad pass and thence southward through La Cañada. These winds strike Pasadena, Mount Lowe, and Mount Wilson from the northwest. The Southern Pacific Railway from Tehachapi to Los Angeles passes through Soledad pass.

The direction of the sea breeze through the San Gabriel valley is of course from west to east. When it is strong and meets a lighter current coming through Cajon canyon from the Mohave desert, it prevails and swings the latter around the San Bernardino mountains, and both then travel on through the San Gorgonio pass into the Colorado desert.

The sea breeze in the San Gabriel valley is usually strong and is felt almost every day during summer. But, quite unexpectedly, on Mount Lowe and Mount Wilson, overlooking this valley, it is always very light. On almost all of the days I spent on these mountains there were sea breezes, very gentle, fresh, but not cold. At such an elevation, with the ocean only a few miles away to the south, and nothing but low hills between, I should have expected much stronger winds from this quarter. The only suggestion I have to offer as a possible explanation is that the mountains to the west and northwest may be a sufficiently diverting barrier to give these winds a course that passes clear of Mount Wilson, and that the higher ridges to the northeast of Mount Wilson form a barrier to currents that otherwise would pass directly to the desert from the sea. It is 80 miles from Mount Wilson westward to the ocean. In this direction protection is afforded by the Verdugo and Santa Monica mountains, which, though lower than the San Gabriel range, are nevertheless sufficiently high to have considerable influence upon the winds. Moreover, it is probable that they help to divert west winds toward the south, causing them to pass to one side of Mount Wilson.

To the west-northwest of Mount Wilson there are many mountains, reaching all the way to Point Conception, 150 miles away; and to the northwest, further inland, are still more extensive ranges. It cannot be doubted that these ranges have an influence on the winds that come from their direction, tempering, deflecting, or even turning them completely aside. In any event, it is pretty certain that they would find an easier passage from the sea to the desert than by passing over the San Gabriel range.

In this connection we may consider the dust in the air over the Los Angeles plains, for this is very intimately connected with the winds that blow from the Mohave desert to the sea. When the desert is dry—as it is during much of the year, and particularly in summer—and strong winds blow over it, clouds of dust are raised high into the air. When this dust laden air passes through Cajon canyon and out to the sea, the air over the Los Angeles plains becomes charged with dust. In general, the dust filled region of the air has a clearly defined upper limit, here called the dust line, having an elevation of from 3,000 to 5,000 feet, sometimes falling lower, at others rising higher, according to the force and height of the winds. The air above the dust line is not entirely free from dust. Here, as in most other places, there is some dust even at very great elevations. Perhaps no place of moderate height in the temperate zones can be found that is wholly free from it.

During my stay in the San Gabriel mountains I watched the dust line closely. It often rose higher than I was accustomed to see it near the coast in central California. It became evident that an observatory at an elevation of less than 4,000 feet would often be below it, but that it does not usually rise much higher than this. Exceptions, however, do occur. At times it rises to 6,000 feet or more. These appear to correspond to the times when great volumes of air are moving from the desert to the sea, volumes so great that they cannot be passed through the dip in the range at Cajon canyon, and must perforce go over the mountains. This happened once during my stay at Mount Wilson. After some hot days in the valley a strong wind came over the mountain from the north, and the dust line, which before had been high, now rose above the peaks in the immediate vicinity. This lasted for a few days, and then the dust cleared from the valley, and the houses in it were again easily in view. Never a day passed while I was there that Grayback and San Gorgonio, the highest peaks in the San Bernardino range, were not visible. Again I could see without difficulty the lower mountains to the southeast—Palomar, the three Cuyamaca peaks, and their lower neighbors, some of them fully 120 miles away. Views such as this are not exceptional. There are some days when the dust rises high and thick, but according to the most reliable information, this does not happen many times in the year, and it certainly did not happen during my stay.

The summit of Mount Wilson is situated in the southwest quarter of section 29, T. 2 N., R. 11 W., San Bernardino base line and meridian. This section is at present owned by the Pasadena and Mount Wilson Toll Road Company, of which corporation Mr. Wm. R. Staats and Mr. J. H. Holmes, both of Pasadena, at present hold a majority of the stock. This company also owns the west half of the southeast quarter of section 30, and the west half of the northwest quarter of section 32, these being the sections which join section 29 on the west and south respectively. This company also owns the toll trail from the foot of Eaton canyon to the summit. All other lands in the immediate vicinity of the summit are owned and controlled by the United States Government, and form part of the San Gabriel Forest Reserve. Messrs. Staats and Holmes, Professors Campbell and Hale of your Committee, and Mr. T. P. Lukens visited Mount Wilson while I was there, and during their stay a number of questions which would arise in practical form if it were decided to place an observatory on Mount Wilson were dis-

cussed. From these discussions it was learned that the present owners would readily transfer a part of the mountain top, including the summit proper, for the use of an observatory; that they would assist in maintaining and fostering the growth of trees and other vegetation; that they would in every way safeguard the purity and permanency of the water supply; and, finally, that they would use their influence to make the summit more accessible by having an electric railway constructed to the mountain top.

The Pasadena trail, owned by the Toll Company, extends from the summit of Mount Wilson to the foot of Eaton canyon, a distance of 9 miles. It is constructed on a grade which is said to be 10 per cent. in most places, but nowhere exceeding this. The trail was located with the idea that it should eventually be converted into a wagon road. This could be done without much difficulty in most places. Portions of it, however, would require rock excavations and the construction of retaining walls, and some bridges might be necessary to avoid some of the present abrupt turns.

The fact that all the land excepting the portions enumerated above is held and controlled by the United States as part of a Forest Reserve is a very important consideration. It insures a constant patrol of the adjacent regions by the forest rangers, a safeguarding against forest fires, which are at times very destructive in semi-arid regions; a regeneration of the forest trees; a saving of the deep accumulation of humus, and thereby a conservation of the water—all matters of vital importance to the well being of an observatory whose work is to be largely solar research.

The San Bernardino mountains have been mentioned as lying to the east of the San Gabriel range, and as culminating in the San Bernardino and San Gorgonio peaks, about 40 miles southeast of Cajon canyon, which separates the two ranges. I did not visit these mountains, but from the time of my arrival in southern California until I came away I made many inquiries and heard much concerning them, particularly from those who have lived in them and made a study of their characteristics. From all the information that I could gather from the most reliable sources it did not seem probable that I should find in these mountains any location which would have conditions favorable to a high grade of astronomical work—conditions comparable to those that exist in the San Gabriel mountains. In reference to these matters I have particularly consulted with Mr. Lukens. In his opinion there is only one region in the San Bernardino mountains that would be at all worthy of consideration, and that would

not have advantages at all comparable with those of Mount Wilson. This place is Rogers cliff, on the Arrowhead grade, near Squirrel Inn Resort. It is said to be a delightful vicinity and one in which lumbering operations have been conducted intelligently. But the land is held in private ownership, and there is no assurance of the timber being renewed, as on the Government reserves. Sheep, which have devastated large areas in other sections, have been kept out of this one. The elevation is about 5,500 feet.

A large portion of the Mohave desert is immediately north of the San Bernardino mountains, and the Conchilla and Colorado deserts lie to the southeast. In the midst of the range, at an elevation of from 5,000 to 7,000 feet, there are valleys of considerable extent, the most noted of which are Bear valley and Little Bear valley. The former lies directly north of the highest peaks of the range and about 10 miles from their summits. The ridges to the north generally rise not more than a few hundred feet above the floor of the valley before they begin their rapid descent into the desert, which here reaches an elevation of about 4,000 feet. In winter snow often covers the ground to a depth of several feet, but when it melts a desert wind may carry away the moisture, leaving very little water. The summer days are warm, or even hot, and the nights are always cold. Even on summer nights it often freezes.

Little Bear valley lies farther west and has an elevation of about 5,500 feet. It has no considerable ridges between it and the Mohave desert, which lies 2,000 feet below it and only a few miles away. Mr. Lukens states that he camped for two weeks in this valley in June, 1899. At that time the temperature rose into the eighties during the day and fell below freezing every night. From temperature considerations alone, it is evident that these valleys would not be desirable locations for an observatory.

The San Bernardino Forest Reserve covers the San Bernardino mountain region, but the Government does not now own or control the best portions of the forested areas. Before the region was set apart as a reserve, private and corporate owners had secured control of practically all the valuable timber in accessible locations. Lumbering operations have been extensive. Logs are sawed at the mills and the lumber is hauled down to the plains. Roads have been necessary for these operations. The steep slopes of the mountains from the plains on the south to the crest of the ridge where the timber and mills are situated made it very expensive to secure easy grades for the various roads that connect the two sections. These are all

toll roads, owned, at least in part, by the same corporations that control the bulk of the valuable timber of the reserve. By making the rates of toll on timber over these roads practically prohibitory, these corporations have made it impossible for any one to conduct lumbering operations except under their auspices or by their permission. In this connection the following statement, taken from a report by Mr. John B. Leiberg* on the San Bernardino Forest Reserve, is of interest:

"There is, however, a point to which I desire to call attention. This relates to the means of communication with the forest areas from the plains. On the Mohave side of the uplift a number of roads and trails lead into the reserve. These roads are free and communication over them with the interior of the reserve is easy. On the western slope the only wagon roads leading into the forested tracts are toll roads owned by private corporations. The section of road from Highland Mill to Bear valley, along the crest of the ridge, is likewise a toll road. These corporations charge persons traveling on Government service toll, or not, according to their pleasure. The Arrowhead Company customarily refunds the toll or gives a pass upon proper application; but not so with the Highland Company, controlling the City Creek Road. Considering the fact that the different corporations own the bulk of the commercially valuable timber which grows in the reserve, and that the Government, in patrolling the reserve in general, protects this timber as well, it would be but a matter of equity that officers of the Government should at all times be entitled to the free passage of these roads."

As a result of the lumbering operations, the roads that lead into these forest regions are much traveled, and in summer they become very dusty. Some of them have much clayey road material, which grinds into very fine powdery dust. Parts of the City Creek trail are of this character. It has been said that "in all the State of California there is not another road so dusty."

I am not in possession of much information concerning the winds in the San Bernardino mountains. It is always stated, however, by those whom I have heard express any opinion in the matter, that winds prevail there to a greater extent than in the San Gabriel range. From what has already been said of the winds in the vicinity of the Cajon pass, and from the proximity to the Mohave desert, we should reasonably expect more wind, at least in the western and more exposed parts of the San Bernardino range, and, along with it, much desert dust high in air.

*U. S. Geol. Survey, Twentieth Ann. Rept. 1898–99, part V, Forest Reserves, p. 454.

To sum up: In the San Bernardino mountains there would in all probability be more wind and more dust, and certainly much greater daily range of temperature and less contact with civilization, than in the San Gabriel mountains. On the other hand, they are much less steep, and already have graded wagon roads to the crests of their ridges.

San Jacinto peak rises 10,805 feet above the sea. On its eastern side it is said to be the highest vertical wall in the United States. From the summit one looks over the San Bernardino plains to the west and over the Colorado desert to the east.

Strawberry valley lies southwest of the peak, half-way down, 5 miles away in a straight line, but 12 miles by the trails that one must take. It is well covered with pines and oaks, and is in the midst of many thousand acres of similar timber. It has numerous springs of pure water and several streams that never fail. The valley is wide and open, and is said to have numerous small elevations within its limits which rise some hundreds of feet above its floor. Tahquitz valley (7,500 feet), Lily rock (7,973 feet), and Tahquitz peak (8,826 feet) overlook Strawberry valley from the east. From what we know of mountain temperatures, we should expect these higher elevations to cause large daily variations of temperature in the valley below them. In themselves these higher levels are too elevated and too inaccessible to be desirable sites for a permanent astronomical station.

Many earthquakes are felt in the San Jacinto mountains. Tahquitz peak is especially noted for them. This circumstance alone counts heavily against the region for astronomical purposes.

I did not make a personal inspection of the mountains in the vicinity of Santa Barbara. They were not considered when the work in southern California was outlined. Later the question arose whether a really fine site for an observatory could be found there, and I then made inquiries concerning them. I have particularly consulted Mr. Lukens, and the salient facts respecting these mountains are given in the following paragraphs:

The Santa Ynez mountain is a ridge several miles in length, about 5,000 feet in altitude, lying parallel to the coast and rising abruptly back of the city of Santa Barbara. Its summit is accessible by a trail, and its air line distance from the sea is only 5 miles. Water is not found anywhere near the summit. There is but little timber on the mountain, and only a fair covering of chaparral—not nearly

so much as there is on Mount Lowe. For many years this has been an extensive sheep range.

The Santa Ynez river lies back of Santa Ynez mountain and is about 2,000 feet below the summit. It is the nearest source of water supply. It is said that the water supply of the city of Santa Barbara will eventually be taken from this river, as soon as the tunnel through the mountain is completed.

Pine mountain is farther inland. It rises to an elevation of about 6,000 feet, and is well covered with timber. The desert lies immediately north of it, and is only about 2,500 feet below the summit. Here there would be no protection from the desert dust and wind. Moreover, it is very inaccessible, as is the case with most of the mountains inland from Santa Barbara. At present there are no roads and no trails over which instruments could be taken.

The mountains north of Santa Barbara are more promising than those to the southeast, and yet they do not have conditions as favorable as one would desire. It is pretty nearly an arid country about Santa Barbara. Toward the southeast it is even more barren than in the immediate vicinity of the city. This is a conglomerate region, in which water is exceedingly scarce.

In the beginning it was not intended that an examination of any portion of Arizona should be made. On his return to Chicago from Pasadena, Professor Hale was much impressed by the transparency of the air over the desert. This led him to urge a brief inspection of northern Arizona. For this inspection I made a trip to Flagstaff and remained a week at the Lowell Observatory—from the evening of July 7 to the morning of July 15.

In response to an inquiry by Professor Campbell, Director Lowell very kindly placed the 24-inch telescope at my disposal, and besides did everything possible to facilitate the work.

In the late afternoon and early evening, Mr. Lowell was using the 24-inch telescope on Venus and Mars. Mr. Slipher was away and the spectroscopic work was discontinued during his absence. This circumstance gave me a large measure of time with the telescope without interrupting the regular routine of the observatory.

I was at Flagstaff at an unfavorable season. For a month or two in the summer there are almost daily thunder showers in this elevated region. Some of these are heavy storms, as was that on the day I left; but more often they are quite local and of short duration. During the week I was there clouds usually formed in the forenoon, sometimes early in the morning, and apparently first in

the vicinity of the higher mountains, such as the San Francisco peaks, etc. In general the clouds covered only a part of the sky, and their drift was slow. Observations were frequently interrupted by them. During the middle of the day the cloudiness increased, and thunder storms followed. Late in the afternoon it would clear and the nights were generally cloudless.

As a result of these unsettled conditions it was not expected that the seeing would be at its best, and during my stay Mr. Lowell did not regard it so. To me it appeared only fair. At no time did I rate the seeing above 3 on my scale. However, a comparison of his standards of excellence and mine was interesting and useful.

I took with me to Flagstaff the helioscope loaned to the expedition by the Yerkes Observatory. Mr. Lowell very kindly had it fitted to the 24-inch telescope, thereby making direct observations of the sun possible. A number of very large sun spots were visible, and much detail in them could be seen at times, but the moments of steady images were of short duration. For the most part heat waves followed each other rapidly, causing much movement of the image and some blurring.

When observations of the sun were made with the large telescope the aperture was reduced by diaphragms placed in front of the objective to 9, 12, or 18 inches.

In addition to the direct observations, the sun's image was occasionally projected upon a sheet of paper, usually to a scale of about 3 feet to the sun's diameter. Most of the observations of the sun were made during the forenoon. The earliest ones were made about 7 a. m., and they were usually continued at intervals until about 1 p. m. On one of the days the sun was also observed about 5 p. m.

Observations of stars were made at night, after Mr. Lowell had completed his work on Mars, usually until about midnight. On the whole, the conditions during this portion of the night were much better than those during the day. Double stars having equal components and distances greater than $0.''25$ were easily separated on most of the nights, and the measurement of much closer pairs would not have been difficult. Nevertheless, the seeing was not at any time of the highest quality. The diffraction pattern of a star's image was always more or less broken and in motion.

During my stay at the Lowell Observatory I detected two double stars, which appear to be new, and verified the duplicity of a third which I had suspected with the 16-inch telescope of the Lowe Observatory. These pairs are as follows:

D. M. $+51°$	1808	$12^h\ 59^m\ 08^s.3$	$+51°\ 46'.8$	Close pair, companion of ε1718.
$+48$	2108	13 13 28 .8	$+48$ 32.6	8.5 and 11.0, Est. $1''$.
$+48$	2243	14 44 15 .6	$+48$ 50.9	A close pair.

While we were at the Lowell Observatory the wind during the day was usually light and always from the southwest. Late in the afternoon it would die down and remain quiet for some hours. This is said to be the normal summer condition. Mr. Lowell states that at Flagstaff the best seeing of the day is usually at this turn of the tide—in the afternoon when the wind is dying down and at the corresponding period in the morning before it rises. This accords with the experience of Mr. A. E. Douglass, who has very kindly placed at my disposal the following statement concerning the seeing at Flagstaff from his forthcoming article on the "Selection of observatory sites":

"The seeing at Flagstaff, Arizona, is dependent on the general and local topographic features and the prevailing winds. The town is situated on the southwest margin of the great Rocky Mountain plateau. On account of the great size, elevation, and barrenness of that plateau, insolation and radiation are stronger than on the other desert regions touching it on the southwest. There is, therefore, an indraft toward this plateau from the southwest in the daytime, stronger in the afternoon owing to the insolation, and an overflow from the desert at night as the cold air settles down upon it, owing to radiation, reaching a maximum in the late night. The normal winds at Flagstaff, therefore, are southwest in the day and northeast at night. In summer, when the days are longer, the day condition prevails, and the prevailing clear-weather wind is southwest (changed to southeast when bringing the summer rains); in winter, with its length of night, the night condition dominates, and the prevailing clear-weather wind is northeast (changed to southwest in a storm area). It is the sunset and sunrise lulls between these day and night winds that give us the periods of best seeing.

"This explanation of the good seeing at sunset and sunrise has been verified in many ways. During the observations of Eros at the Lowell Observatory, when it was necessary to observe at least twice during the night, once early and once late, the late seeing on clear nights was always worse than the early, and it was usually accompanied by a current of air from the northeast. This is the overflow from the desert, as already explained. This overflow, coming on rather suddenly late in the night, is also the explanation of the sudden rise in temperature in the valley of Flagstaff in the summer, at three or four o'clock in the morning, frequently found on the thermograph records. Previous to this hour the quiet air in the valley of Flagstaff has been growing steadily colder; but when the overflow occurs in the form of a fresh northeast wind it

carries the settled cold air out of the valley and raises the temperature.

"The southwest margin of this desert plateau is a ridge called in part the Mogollon mesa. It follows the line of a great fault through Arizona from northwest to southeast. This ridge acts like a dam to the rising warm air by day and the down flowing cold air at night. Flagstaff is at one of the low points on this dam, and therefore gets a particularly pronounced effect from this interchange of air between the high and low desert regions. Its winds are probably stronger than those anywhere else along the ridge and they begin earlier and last later.

"Now as the cause of unsteady seeing is the multitude of irregularities of density in the air, produced by contact with heated or chilled surfaces, and as, also, these irregularities are brought over the telescope by aid of the wind, we may generalize and say that the badness of seeing increases with wind and the opportunity that wind has of contact with heated or chilled surfaces. We have seen that Flagstaff is particularly subject to strong winds as compared with other parts of Arizona; we have now to study the opportunity that wind has of contact with heated or chilled surfaces. It is perfectly evident that hills and mountains, reaching up into the great mass of moving air, present the best opportunity for such contact. On examination we find that the entire horizon of Flagstaff from north-northwest to east-northeast is occupied by a mountain range, rising from 2,000 to more than 5,000 feet above the town, and distant from it between 4 and 12 miles. The winds that pass these peaks are not only diverted in a vertical direction, which causes change in their density, but also, by contact with their warmed or chilled surfaces, suffer small irregularities in density throughout their mass. Hence, whenever the wind blows from a northerly direction the seeing becomes poor in proportion to the velocity of the wind and its directness of passage over the mountains. Therefore in winter, when the northeast wind predominates, the seeing is generally poor.

"This explanation of the bad seeing in winter has been frequently verified by testing on the same night the atmosphere at Flagstaff and at points as little as 12 miles east, out of the lee of the mountains. Even when the seeing at Flagstaff was frightfully bad, Eros showing a diameter of 13 seconds, the seeing away from the mountains was perfectly fine and steady.

"Thus the times of best seeing at Flagstaff are at sunset and sunrise in the non-winter months, and the poor seeing at other times results from the topographic features and the prevailing winds.

"It should be remembered that this criticism of Flagstaff seeing is derived from a minute comparison of it with other parts of the southwest arid regions. Compared with eastern localities I regard its qualities as very superior."

I took with me to Flagstaff one of the Draper thermographs belonging to the equipment of this expedition, and during the week

I exposed it in the thermograph shelter of the Lowell Observatory. The range of temperature shown by the record was 29°, from 59° to 88°. The greatest variation in any one day was 24°.

The record during the day was generally full of small variations, in addition to the larger changes. This was probably due to the passing of more or less extended clouds, giving intervals of sunshine and of shade, but no direct test of this hypothesis was made.

The temperature at night was more uniform than by day, but the night curve did not always have the smoothness that one likes to see. However, on some of the nights there would be an interval of five or six hours during which the change of temperature would amount to only one or two degrees.

Mr. Lowell very kindly placed at my disposal the thermographic record of the observatory. The time available did not permit a detailed study of the entire record, but I examined with some care the sheets for the year 1902.

Some of the general characteristics of the record were at once evident. The fluctuations during the daytime were often large, and superposed upon these were frequent minor variations. In passing from day to night temperatures there was, as is always the case, a pretty rapid change during the early hours of the night. This period usually lasted about three hours—to take the year through, from about 6 p. m. to 9 p. m., but earlier in winter and later in summer. This, however, was not the rule. After the early evening hours the temperature gradient would generally become less steep, or even nearly horizontal, save for minor fluctuations.

A feature worthy of special notice was prominent on many of the sheets. Often in the latter part of the night, generally after 3 a. m. and often as late as 6 a. m., the temperature would suddenly fall from 5° to 15°, and then after a short time rise again, partially regaining its former height. As an example, on the sheet I obtained there is shown on the morning of July 14 a sudden drop of about 7° shortly after 3 o'clock a. m., and half an hour later a rise, nearly as sudden, of about 4°. I have been told that these sudden changes of temperature are here associated invariably with very bad seeing, which is entirely in accord with what might be expected under such circumstances.

Respectfully submitted.

W. J. HUSSEY.

MOUNT HAMILTON, CAL.,
August, 1903.

APPENDIX B TO REPORT OF COMMITTEE ON OBSERVATORIES

LETTERS RELATING TO SOUTHERN AND SOLAR OBSERVATORIES.

CONTENTS.

I. CORRESPONDENCE RELATING TO PROPOSED SOUTHERN OBSERVATORY.

Extracts from Confidential Statement.

The following extracts from a confidential statement, prepared in June, 1903, are prefixed to the letters relative to the proposed Southern Observatory, in order to render more intelligible the comments upon the statement contained in the letters:

"The Advisory Committee on Astronomy for last year of the Carnegie Institution (Messrs. Pickering, Langley, Newcomb, Hale, and Boss) recommended the establishment of two observatories. One of these was proposed for the southern hemisphere, to assist in reducing the disparity which now exists relative to the observation of astronomical objects in the two hemispheres. The other is a proposed solar or astrophysical observatory, to be established in the best practicable atmospheric conditions and with a powerful equipment.

"The Institution is not in any way committed to either of these enterprises at present. * * * Accordingly a special commission of three astronomers (Messrs. Campbell, Hale, and Boss) was appointed to investigate and report more fully in regard to these recommendations. * * *

"Among the special observations that are regarded as important are the following:

(1) "A fundamental determination of star positions, with extension of these observations by secondary methods to include every star brighter than the seventh magnitude between —20° and the south pole. * * *

(2) "Observations for stellar parallax have been secured at the Cape Observatory. Against this one observatory we find several observatories in the northern hemisphere actively engaged in deter-

minations of stellar parallax. * * * Therefore, determinations of these has been considered an important feature for the work of the proposed observatory.

(3) * * * "It is considered highly desirable that another very large reflector should be provided for this use [determination of radial motion] in the southern hemisphere. Were such an instrument provided it could also be employed in other spectroscopic researches requiring powerful optical means and also in the photography of nebulæ. * * *

(4) "The extension from —32° to the south pole of zone observations upon the general plan adopted by the *Astronomische Gesellschaft*. The Cordoba Observatory has secured the necessary observations for the zone —22° to —32°. Only one fourth of the sky remains to be covered. * * *

(5) "It is thought desirable that extensive observations for variations of latitude should be made in the southern hemisphere somewhat upon the plan now adopted for the northern hemisphere in the international service. * * *

(6) "During recent years, and especially at the Lick Observatory, very thorough surveys of the stars in the northern sky have been carried out for discovery and measurement of close double stars. It is considered desirable that similar investigations should be carried out with at least one large telescope for that part of the southern sky which is beyond the reach of northern observatories. * * *

(7) "Eventually certain astrophysical researches of precision, requiring great optical power, will be required in the southern hemisphere to complete the evidence relative to certain classes of objects which have been, or are about to be, investigated in the northern hemisphere. * * *

"In view of these considerations, it would be of great service if the consensus of experienced astronomical opinion could be obtained in regard to the relative urgency of observations which ought to be undertaken now in the southern hemisphere and which are inadequately provided for at existing institutions there, whether such observations have been alluded to in this statement or not. * * *

"It would also be useful to learn whether any similar plans are known to be under consideration by others; or whether there is any prospect of such. * * *

"Letters have been addressed to astrophysicists in relation to the proposed astrophysical observatory for which a site in southern California is contemplated. The object of this observatory would be to investigate the physics of the sun, and, especially, the amount and variation of solar radiation, with apparatus more powerful than, and in conditions of atmosphere superior to, any which have been hitherto available for the purpose. Your suggestions and advice upon this head would also be very acceptable." * * *

LETTERS FROM CORRESPONDENTS.

[*From Director H. H. Turner, of the University Observatory, Oxford, President of the Royal Astronomical Society.*]

OXFORD, *July 17, 1903.*

The Statement deals chiefly with the project for a Southern Observatory, and this reply will be confined to that project. Before considering the points of detail raised in the statement I should like to express an emphatic opinion that there is now, as there has always been hitherto, a great need for assistance of astronomical observation in the southern hemisphere. Various spasmodic attempts have been made in the past to bring our knowledge of the southern hemisphere into better relation with that of the northern. Some of these have been successful, others have failed: and it will help us in considering the matter if we glance at a few instances of both kinds. As unsuccessful attempts I will quote—

1. The project initiated by a Committee of the Royal Society half a century ago (see Mon. Not., R. A. S., XIV, 129) for a large reflecting telescope in the southern hemisphere. The Melbourne telescope was provided, but very little has been done with it.

2. The project for a Southern Survey (see Mon. Not., XXIII, p. 147; XXV, p. 118; XXVII, p. 132; XXIX, p. 161) which seemed to be making some progress, but eventually died out.

3. The southern share of the Astrographic Chart, which is lamentably behindhand at present; three of the southern observatories which originally undertook a share never made a start, and two of those which replaced them are in great need of assistance. At others the work is going on very slowly, except perhaps at the Cape Observatory.

For comparison with these we may cite various successful enterprises, and it will be seen that they are either of a purely expeditionary character or that at least the workers remained in touch with the northern hemisphere.

4. The expeditions of Halley and Sir John Herschel to the southern hemisphere, which successfully advanced our knowledge of that hemisphere at the epochs of the expeditions.

5. The expedition, in recent times, of Mr. McClean, who extended his spectroscopic survey of the bright stars to the whole sky and incidentally discovered oxygen in β Crucis.

6. The semi-permanent expedition from Harvard to Arequipa, the material collected at Arequipa being returned to Harvard for examination and discussion. Among other discoveries, this has given us several "new stars."

7. The work done by the late E. J. Stone in forming the Cape Catalogue, 1880, which was largely expeditionary in character. Mr. Stone remained at the Cape only 9 years, and went out definitely to do this single piece of work.

8. To some extent the same may be said of the work done by Dr. B. A. Gould at Cordoba.

9. In considering the eminently successful work done by Sir David Gill at the Cape Observatory, it may be remarked that he has paid frequent visits to Europe and kept himself in close touch with the northern hemisphere throughout. In the case of one large piece of work, the Cape Photographic Durchmusterung, the measurement of the plates was conducted by Professor Kapteyn in Europe.

No conscious selection of the above nine instances has been made, though it is quite possible that I have been unconsciously influenced by the opinion already indicated: that much, if not all, of the successful work done in the southern hemisphere has been accomplished either by observers who have made a purely temporary expedition from the northern hemisphere, or by men who have kept closely in touch with the North, or quite recently by the examination, in Europe or America, of material collected in the southern hemisphere.

And if this is true, it is, after all, only natural. The permanent observatories in the South have to contend with the gravest disadvantages. The governments at their backs are comparatively poor; there are heavy claims upon them for other work, such as meteorology (as at Sydney), or telegraphy (as at Adelaide), and they are far removed from the advantages of the older civilizations in respect of intercourse with men of science.

The lessons inculcated with regard to future projects are tolerably obvious, and I cordially welcome the general proposal to establish a Southern Observatory of the *expeditionary kind* indicated in the statement. I proceed to consider the points of detail dealt with in the statement.

The Observations Considered Important.

1.* *Urgently Needed.*—Personally I should like to see the photographic method used. I have elsewhere indicated (Mon. Not.

* The figures refer to the numbered paragraphs in the Confidential Statement, pp. 106–7.

R. A. S. LVII, p. 349) the lines on which I feel sure a simple and efficient photographic transit circle could be constructed. The heavy work of the Astrographic Chart has compelled me to defer any trial of this method, but having nearly completed our measures we are now on the point of trying it, and possibly information as to its success may be forthcoming at an early date. The "magnitude equation" is so troublesome for eye observations that I feel sure we ought not to delay the introduction of photography into meridian work any longer, and I feel confident of the success of the method suggested. No new departure is contemplated beyond mounting a mirror on pivots instead of providing a telescope, and this should offer no difficulty—rather the contrary. But I am well aware that no instrument is complete so long as it is only on paper.

2, 3, 4, 5, and 7. I thoroughly agree with what is said and have no special remarks to make.

6. *Double Stars.*—The work of Mr. R. T. A. Innes at the Cape Observatory should be remembered; but Mr. Innes has recently been appointed director of a new observatory where this work may not be possible, since the first call on his energies will be from the *meteorological side.* Compare what has already been said about the non-astronomical calls on the attention of directors of southern observatories.

Other Observations not Mentioned in the "Statement."

8. *Planetary observations of position* from the southern hemisphere are in some ways as desirable as stellar. Sir David Gill has already started observations which will supply several of these wants; but I should like to draw attention to the fact that a series of observations of the moon which would give good places *throughout the lunation* during at least one year would certainly improve our knowledge of the parallactic inequality; and for completeness we should have series obtained at both northern and southern stations, unless a series could be obtained from near the equator, as Madras or Kodaikanal. The chief requisite is a *fine climate*, such that the moon could be caught very near new moon and pretty continuously in at least one or two months.

There are various methods which might be used; to give one example, photographs might be taken with a telescope clamped for the night (*a*) of the moon and (*b*) of stars preceding or following by a known interval. These photographs could be measured and discussed elsewhere if necessary.

9. At some time a number of longitudes of stations in the southern hemisphere must be determined with modern precision.

While not immediately pressing, this work might become pressing at any moment; and it could scarcely fail to be of ultimate service if a new station were fixed relatively to (say) the Cape Observatory with accuracy.

Similar Plans under Consideration.

I know of no similar plans. On the other hand I have had special reasons for fearing that the total available energy of southern observatories will for the next twenty years be almost completely absorbed by the work of the Astrographic Chart. On this point I have already addressed a letter to the Chairman of the Committee, and perhaps I need not here repeat what I said.

Location.

I have made inquiries about

(*a*) *South America*, which would in many ways be suitable, as differing in longitude from existing observatories. For stations of considerable southern latitude the replies have not been favorable.

(*b*) *South Africa.*—I hear very good accounts from Sir David Gill and Mr. Innes of the site selected by the latter for the new observatory of the Transvaal.

(*c*) *Australia.*—Mr. Russell gives good accounts of sites near Sydney. Recently I have heard sites near Hobart Town, in Tasmania, much commended. The climate is said to be beautiful and land so cheap that a large tract could be secured.

General Plan of the Work Temporary.

I have already commented upon this point at the beginning of this letter. To keep in view the "accomplishment of a few specific works, the conclusion of which could be foreseen within a limited term of years," seems to me to be exactly the way to set about the advancement of the astronomy of the southern hemisphere, if we may trust previous experience; and it seems to me further quite probable that there are several men, perhaps many men, of proved capacity and experience, who would find an expedition to the southern hemisphere desirable and delightful if it could be undertaken without financial loss and for the completion of a piece of work already carried out in the northern hemisphere. I understand that this kind

of expedition is in the minds of the Committee, and it seems to me to be most attractive, and to promise an economical solution of the problem of dealing adequately with the southern hemisphere on lines suggested by a study of past history.

[*From Director M. Loewy, of the Paris Observatory, President of the Congress for the Carte du Ciel.*]

[Translation.]

PARIS, *July 20, 1903.*

The establishment of two observatories in the southern hemisphere would be, without any doubt, of high utility for astronomy.

The program prepared by the Committee in regard to the work to be accomplished there, seems to me, in its broad lines, most judicious. You have expressed in your communication two ideas which are in perfect harmony with my own personal view: First, that large scientific enterprises often stand in intimate relation with each other, which brings it to pass that the success of the one instigates progress in the others; Second, that it is desirable to have at our disposal a large number of carefully determined proper motions in order to undertake with profit some of the noblest problems which today are occupying the minds of the most eminent astronomers. It is only in this way that one can acquire more precise conceptions of the nature of the trajectory of our solar system and of the general structure of the universe. I take this point of view in communicating to you the reflections suggested by a perusal of the program of the committee.

First of all, I should like to suggest a slight modification of your plan of work concerning the execution of the meridian observations between $-32°$ and $-90°$ of declination, in order to give to this research a more general utility and to render it valuable in the construction of the Astrographic Chart. In order that you may better appreciate the drift of the remarks which I submit to you, allow me to present some explanations concerning the details connected with this work. The recent researches which I have published on the subject of the precision with which one can take the astronomical coordinates of the stars from a photograph show that by exercising the necessary precaution one can attain such precision that the total probable error would reach at the maximum $\pm 0.''2$ even for the faintest images. This determination is not in the given instance an *optimistic* estimate; it represents the reality. It is well to add that

a *great part* of the probable error above indicated springs from the inexactitude with which the positions of the comparison stars, chosen for the calculation of the constants of the plates, are affected.

From this one may conclude that if the photographic observations should be repeated twenty years from now under the same conditions of precision we should be able to determine with accuracy the proper motions of about 100,000 stars, as I have indicated on a former occasion.

Besides, in that which concerns the construction of the Astrographic Chart, which includes the exact positions of more than 3,000,000 stars, twelve observatories out of the eighteen in this line of work have not only completed their observations, but have also concluded the task of measuring the rectilinear coordinates that correspond to them.

One of the essential requirements under which the degree of approximation mentioned above could be equaled, and even surpassed, lies in the determination of the plate-constants with all possible accuracy. In investigating this question, the majority of the observatories associated in this work have decided that, if the positions of the comparison stars should be taken from the catalogue of the Astronomische Gesellschaft, their accuracy would be insufficient and would not correspond with that which obtains in the measurement of the rectilinear coordinates. It is for this reason that out of the twelve observatories above mentioned seven proceeded to the direct determination of their comparison stars with the help of precise meridian observations.

It seems to me, then, that the general catalogue which you are proposing to observe between —32° and —90° would have a much higher value if, aside from all other applications, it could supply the comparison stars necessary for the reduction of the plates of the Astrographic Chart included between —32° and the south pole, entrusted to the observatories at the Cape, at Melbourne, Sydney, and Perth (western Australia).

Influenced by these considerations, Sir David Gill has already determined the positions of 8,556 comparison stars of his zone by very careful meridian observation—about twelve for a plate and three meridian observations at least for each star.

To fulfil the requirements for the chart of the heavens relative to this region, it would be necessary to obtain the accurate positions of about 30,000 stars, which would give about twelve comparison stars for each plate of four square degrees.

Instead of observing in an expeditious way, according to the methods recommended by the Astronomische Gesellschaft, 60,000 stars, requiring, according to your estimate, about 150,000 observations, I would prefer that the positions of 30,000 only should be determined with greater accuracy. This would possess the double advantage of giving, on the one hand, a very precise basis for the photographic catalogue of this part of the sky, and, on the other hand, of providing a solid foundation for determination of the proper motion of the stars. It is very desirable that we should have at our disposal, at an early day, the results for this element, which is of such fundamental importance in modern astronomy. With this object in view, to make observations with twice the degree of accuracy is to reduce by one half the interval of time necessary to render sensible the proper motions of the stars.

It would also be desirable, in order to place the catalogue on a sure and homogeneous foundation, to combine it with the 6,000 stars above the seventh magnitude which you propose to determine with the most rigorous precision. A catalogue constructed under these conditions would certainly serve in the future as the starting point for all researches of high precision concerning the study of proper motions between —32° and —90°.

Before leaving this subject I will present another example to illustrate its great value. In addition to the plates of short exposure intended for the construction of the Astrographic Chart, comprising all stars down to the eleventh magnitude, it is also intended at the same time to secure plates of long exposure, guarded by the same precautions and including stellar images down to the fourteenth magnitude. Then, if necessary, the plates of this second series could be measured and reduced with the same degree of precision as the first. Since a great many of the plates contain the images of 1,000 to 12,000 stars, one can readily see that, after photographing for a second time one of these regions so rich in stars, the comparison would reveal a very large number of proper motions for all grades of magnitude down to the fourteenth, a feature of this plan which might prove very instructive from the cosmogonic point of view.

In your program you point out the necessity of determining the parallax of stars; the value of such work is incontestable.

In my opinion the surest method and the one least subject to systematic error is the photographic method properly applied. It would therefore be very judicious to provide for this purpose a

photographic instrument similar to that used for the Astrographic Chart. This instrument could also render valuable service in other lines of work.

Among the eighteen observatories that were associated in the work of the Astrographic Chart there is one that has not held to its engagement. This is the observatory at Santiago, Chile. It is in order to fill its place. You could furnish invaluable assistance to this vast scientific enterprise should you also devote this instrument to the work of the Astrographic Chart.

In a favorable climate an experienced observer, aided by two assistants, could accomplish it in about eight years and could secure all the plates for this zone—1,260 plates for the catalogue and 1,260 for the chart. This period of time should suffice, now that all the methods are so well established.

In providing the catalogue of comparison stars, or by undertaking the vacant zone, or, better still, by accomplishing both tasks at the same time, America would find her opportunity to occupy a most honorable place in this vast international enterprise, whose success, which is now assured, will be without the least doubt the occasion of numerous and noteworthy discoveries.

Finally, it seems to me proper to mention a very useful and inexpensive work which could be undertaken by one or the other of the two proposed observatories, and which up to the present time has been wholly neglected in the southern hemisphere—the systematic observation of meteors. An experienced astronomer, with the aid of one assistant, in three or four years would be able to produce important results in this line. It is in respect to the discovery of stationary radiants, the activity of which lasts for weeks and even months, that he would supply most valuable material for the study of the causes and conditions which produce this curious phenomenon. On the other hand, one could also reasonably hope to run across some notable swarm of meteors in connection with the periodic comets, examples of which, still so rare (four in all) have been observed only in the northern hemisphere. This would be a particularly desirable contribution, which might render possible a more searching investigation of the close relation existing between comets and meteors; also comparison of the frequency with which meteors appear in different seasons of the year cannot fail to bring enlightenment on the origin and velocities of meteors.

In that which concerns the location of the astronomical station the latitude should certainly not be north of —30°, as you have

indicated. It would be advisable at the same time to seek for a climate as free as possible from dust and cloudiness. Perhaps a desirable location could be found in the Argentine Republic, preferably a little south of Cordoba. The climate of that city seems to have lost in quality during the last few years; the winds filled with dust have become more frequent.

[*From Director H. Bruns, of the Royal Observatory, Leipsic.*]

[Translation.]

LEIPSIC, *July 23, 1903*.

In answering the questions proposed by you, permit me first to formulate the following propositions: Astronomy, now and in the future, is confronted with the task of attaining in reference to the universe of stars what has already been accomplished for the solar system, namely, a sure knowledge of the space arrangements and of the motions of the bodies therein contained. Furthermore, the duty is imposed upon each generation of astronomers to contribute toward the attainment of that end whatever is possible with existing means.

If, now, one reviews what has been already accomplished in this field, it will be seen that the weak point is not the extent and quality of available observations, but the unequal distribution of them upon the two halves south and north of the equator. From this inequality arises a sensible defect in all discussions which have for their subject, not single objects or groups of objects, but the heavens as a whole. Therefore, without any reservation whatever, I agree with you that an attempt should be made to lessen this defect, especially since an effective remedy through increase in the permanent observatories of the southern hemisphere is not otherwise to be expected at present.

As to that which concerns the individual propositions set forth by you, I am of opinion that the strengthening of the weight of exact star positions has precedence over all other things. Observations for parallax, velocity in the line of sight, etc., can be secured at any time, whenever the means are at hand, without fear that a marked disadvantage to the development of astronomy would result from such delay. On the other hand, there is one thing that cannot be retrieved through later observations, and that is the "epoch" of a catalogue.

Of the catalogue works mentioned in your communication, the extension of the zones of the Astronomische Gesellschaft to the

south pole is, in my eyes, the one most important for the future of astronomy. This assumes in advance, however, that the work designated by you as No. 1, for the brighter stars, should precede. Therefore, I entertain no doubt that proposition No. 1 should have the preference over all the other tasks.

[*From Director Küstner, of the Royal Observatory, Bonn.*]

[Translation.]

BONN, *July 27, 1903.*

I have read your letter of July 8 and the enclosed program of the Committee of the Carnegie Institution with the greatest interest, and I hasten to express to you my full and unqualified acquiescence in the propositions therein contained.

The situation is so clear and simple that, in my view, only one answer is possible to the question, "How can astronomy be promoted to the best advantage?" namely, through the establishment of an observatory equipped with the best instruments in the most favorable location in the southern hemisphere. The present neglect of the southern sky is felt in the most troublesome manner in all astronomical problems, and many series of observations that have been secured in the northern sky with great care and at great expense cannot be fully employed for the benefit of science because they pertain to only a part of the sphere. The most important conclusions can be reached only after these have been equally extended over the southern sky, and then only will the finest fruits of astronomical investigation begin to ripen.

I can but join in approval of the list of works, arranged in a preliminary way according to their importance, which is proposed for this Southern Observatory. This list might easily be further increased, but practically, at the outset, it may have to be curtailed.

I hold point 1 [meridian observation of stars down to the seventh magnitude] as the most important and—because closely related to it—point 4 [observation of all stars down to the ninth magnitude, southward from —32°]. The prompt continuation to the south pole of the great undertaking of the Astronomische Gesellschaft, which has already been extended down to —23° or —32° is an unconditional necessity. This last quarter of the sky must soon be worked out, if we are not to lose a considerable part of what has been accomplished in the three quarters already completed.

In the continuation of this plan of the Astronomische Gesellschaft

one will naturally profit by the experience which has been gained in the meantime. For example, the personal equation for magnitude should not be simply determined in an incidental way, but directly eliminated. Further, the zero-stars should be observed not merely at the beginning and end of the zones, but they should be regularly distributed throughout the zones, in such a manner that they should be observed at average intervals of about ten minutes.

For the next most important works I consider points 2 and 3 to have the preference [point 2, determination of stellar parallax; point 3, determination of the velocity of stars in the line of sight].

As to the question of choice of a suitable site, I am unfortunately unable to make any definite recommendation. The precision, and consequently the value, of astronomical observations is dependent in so large a measure upon the condition of the atmosphere, that, in the choice of site, regard for the best possible conditions of atmosphere must be the controlling factor, especially when an observing station is to be established for a few years only. If the question turns on the establishment of a permanent observatory, then, by all means, proximity to some center of civilization is also to be considered.

Furthermore, I can but agree in the most complete conviction with the proposition for a great astrophysical observatory especially for solar investigation. The only scientifically correct and at the same time practical way to attain the beginnings of knowledge as to the nature of the fixed stars is, in the first place, through most exact investigation of the star nearest to us, the sun, concerning whose constitution so many obscure problems prevail.

I hope that the Committee may be successful in making the large resources of the Carnegie Institution useful for astronomical investigation in the manner proposed.

[*From Director H. Seeliger, of the Royal Observatory, Munich, President of the Astronomische Gesellschaft.*]

[Translation.]

MUNICH, *August 3, 1903.*

The establishment of a new observatory in the southern hemisphere, with a large provision for instruments, must be regarded as a desideratum for astronomy. The opinions of astronomers will not differ in this respect, and it would be quite unnecessary to prove this in detail, especially since the Statement which you have been good

enough to send me handles the question completely to the point, and exhaustively. The problems that fall to such an observatory arise through the development of modern astronomy, and, indeed, in this Statement they are so fully and clearly enumerated that very little can be added. The main point is to establish the requisite balance between observations made upon the northern and the southern hemisphere, which, hitherto, the few southern observatories could not possibly maintain. Any arrangement, according to their importance, of the classes of observations that ought to be made will naturally depend upon the standpoint of the one who renders judgment, and therefore I can only say that I hold the order given in the Statement to be essentially sound. On the other hand, one can make good the claim that the astronomer has not only to collect the knowledge which will enable later generations to derive important results, but also that he should especially challenge those problems that will permit general conclusions to be drawn within a time not distant, even though in a fragmentary way only.

From this point of view I have for several years expressed the opinion that, though observations of fixed stars made to establish the *motions* of the heavenly bodies are important beyond doubt (though they will first bear fruit in the distant future), yet observations concerning the *present aspect* of the starry heavens should not be neglected. This aspect has an independent interest of its own, and from it a valuable result can be drawn at once without waiting for the cooperation of future generations. Accordingly, on account of their bearing upon my own investigations in regard to the space relations of the stellar system, I would like to designate as especially important the following problems for which the cooperation of the observatories in the southern hemisphere is absolutely necessary, since it is possible to make these discussions only upon the basis of observations distributed over the *entire* sky:

(1) Determination of parallaxes, which appears as (2) in the Statement.

(2) An investigation of the apparent distribution of the stars in the southern hemisphere, including those of the faintest magnitude that can be observed. The Statement excludes the consideration of new problems, to be sure; but here we are concerned, not with a new work, but with an old one which has not hitherto received sufficient attention, the object of which is to solve the new problems more conclusively and the execution of which would, in effect, establish the solutions upon a sounder basis. A similar work is now

being carried out for the northern hemisphere at the Munich Observatory, though in a very modest way, since the means at its disposal are very limited, and in this connection I would refer to my report in the Vierteljahrschrift of the Astronomische Gesellschaft.

(3) Very many—indeed, most—of the investigations in stellar astronomy depend upon the establishment of an exact photometric scale of magnitudes. For the northern sky, in this respect, the magnitudes of the stars down to about the eighth magnitude are fairly well established through the labors at Cambridge (Mass.) and Potsdam. In this connection is to be noted the demand for fixing by photometric methods the magnitudes of the fainter stars, through a satisfactory choice of objects, evenly distributed, down to the faintest which can be observed. This is still an object to be desired. For the brighter stars in the southern hemisphere an extensive series of observations is available. But undoubtedly this is not free from objection, and we are not in position to establish the distribution of the stars upon the southern sky in combination with that upon the northern hemisphere.

The Statement does not propose to take up photometric and photographic works for the present at least. But I think that the works designated under (2) and (3) are those that are now most pressing and, at the same time, those that would be most acceptable. Here, within a few years, if sufficient means exist, we may hope for results that will be of the greatest importance for all time. Of course, one would better observe with the same instrument first at a station in the northern hemisphere, and then from a station in the southern hemisphere, or one would have to choose a place in the neighborhood of the equator, and from that point survey the entire sky, which, of course, would not be entirely free from objection.

The choice of a place for an observatory I consider to be extremely important. When one reflects what limitations and disadvantages the climate in our latitudes impose upon all astronomical observations, one can only look with envy upon the astronomer who is permitted to live in a good climate. What a mass of provocations and waste of time is such an astronomer spared! I take this opportunity to call your attention to the Australian continent, or to Tasmania, whose wonderful and, at the same time, healthy climate I know from my own experience, though from no more than a residence of a few months.

If, in conclusion, I may present an arrangement of the works mentioned in the Statement, according to my opinion of their im-

portance in relation to the views I have expressed in the foregoing, I would arrange them thus:

1. Point (2) of the Statement (parallaxes).
2. Point (3) of the Statement (radial motions).
3. Point (1) of the Statement (meridian observations).
4. Point (4) of the Statement (zone observations).

Your further arrangement corresponds perfectly to my views.

At all events, a great and inestimable gain for astronomy would be realized if your views in reference to a new Southern Observatory should come to fulfillment, and it appears to me that in that event it is not so much a question *what* important works shall be assigned to the new observatory, but rather that there are in general important works which it has to accomplish, and that all in the Statement are such I am fully convinced.

[*From Sir David Gill, Astronomer Royal at the Cape of Good Hope.*]

CAPE OF GOOD HOPE, *12th August, 1903.*

You ask on behalf of your Committee my views on the subject of the most urgent needs of astronomy.

There cannot be the slightest doubt that from the highest standpoint what is most urgently required is an increase in the astronomical equipment of the southern as compared with the northern hemisphere, and this is equally true in the departments both of the older astronomy (astrometry) and astrophysics.

There are urgent needs in both of these departments. The relative urgency will vary, in the opinion of many, according as the individual's knowledge or sympathy lies with one department or the other.

Astrometry.

A, 1. In connection with the older astronomy, I entirely concur that the establishment of an additional meridian circle of the very first class in the southern hemisphere in an ideal observatory for fundamental observations is a first essential. Practically the Cape is the only observatory where really fundamental work is being undertaken, and some independent check or comparison is necessary if only to give assurance of the accuracy of results arrived at.

It may be remarked that most of the observatories of the northern hemisphere are defective in the form of covering or observatory for their transit circles and in the means of equalizing the internal with

the external temperature. Therefore, if any transit circle is to be sent to the southern hemisphere, and is to be used before or afterward in the northern hemisphere for the purpose of comparing results, it seould be provided, for both series of observations, with a modern steel observatory having double or triple walls, means of conveying the convection currents away from the observing opening, and of separating the whole observatory into two halves, so that the instrument may be used as nearly as possible in the open air.

There is a still more important condition which should also be fulfilled, viz., provision of means to avoid personal equations depending on magnitude or upon the velocity of the star's motion (*i. e.*, the star's declination). It seems to me that the only system of observing in R. A. which permits this possibility is the Repsold-Struve method, in which a wire is made to travel across the field at nearly the same velocity as the star. The eye piece travels with the wire, so that, if the mechanical conditions are properly realized, the observer, having bisected the star disc with the wire, should view the disc so bisected apparently as if at rest, and be able by simple means to correct any errors of this bisection which he may notice during transit and which may be due to errors of the clock-work, the driving screw, or the original pointing. The drumhead of the screw which causes the slide of the moving wire to travel is provided with contacts which, as the drum rotates, make electric contact with the chronograph circuit, and so record the instants when the pointing on the star would correspond with the particular readings of the micrometer head.

This method, so far as I am aware, is the only one not liable to personality depending on magnitude or declination, and this, although we have not yet absolute proof, we believe also to be free from personality in observations of the sun and moon.

The necessity for provision of reliable azimuth marks and of a clock not liable to diurnal variation of rate is too well known to require further reference.

A separate memorandum dealing with some details of the above-mentioned methods will be forwarded.

As to a site, I think it would be difficult to find in the southern hemisphere a better one for this purpose than the neighborhood of Bloemfontein. I venture to think that in connection with this plan one or two of the northern observatories should be provided with a better form of observatory or covering for the transit circle.

A, 2. There are two other problems of the older astronomy which cry out for solution, one of which is a comparatively small affair, the other a very big one, but both are urgent. I refer to the completion of the organization in the southern hemisphere for determining change of latitude (that is the smaller affair), and to the formation of a parallax Durchmusterung—*i. e.*, determining the parallaxes of *all* the stars to a certain order of magnitude—(that is the large one).

I take the simpler matter first. Mr. Chandler proposes a southern belt of observatories:

	Lat.	Long.
Sydney	$-33°\ 51'$	$-151°\ 2'$
Cape of Good Hope	$-33\ \ 56$	$-\ \ 18\ \ 5$
30 miles south of Santiago	$-33\ \ 54$	$+\ \ 70\ \ 7$

It should involve very little trouble and comparatively small expense to establish the necessary organization at Sydney and the Cape, and I venture to think that the Carnegie Institution could not be better advised than to provide at once for the observatory near Santiago, equipped with two observers devoted wholly and solely to determination of the aberration constant and change of latitude. The instrument I would recommend for all the three observatories would be the photographic almucantar and the method used by Cookson (see Monthly Notices, R. A. S., LXI, p. 315). There should be observations of every group of stars in the early evening, the early morning, and near midnight, at all times when opportunities occur. In this way we ought to get an extremely accurate determination of all the latitude changes and a powerful determination also of the aberration constant.

A, 3. The parallax Durchmusterung is a very much greater undertaking, but it is of the supremest interest to science. I do not think it desirable to go beyond magnitude 9½ or perhaps 9; even then the taking and measuring of the plates is a very big business and involves a large organization.

A telescope of large aperture is not necessary, but considerable focal length is requisite to give the necessary precision of measurement. The highest optical perfection should be arrived at, probably a 4-glass objective of 8 or 10 inches aperture and 20 feet focus would be most suitable. I think it very undesirable to employ a cœlostat or any plan involving reflection from a plane mirror, as plane mirrors may be liable to flexure or deformation by temperature changes.

Kapteyn's method should be adopted, and all the photographs taken at different seasons on the same plate should be exposed at the same hour angle. Although this involves some sacrifice of parallax factor it eliminates many possible sources of systematic error. I would advocate an observatory (that is to say, a telescope and two photographic observers) devoted exclusively to this work, with an office and staff of measurers and computers located elsewhere, where the services of students and others could be secured, living is less expensive, and facilities for instrumental construction and repair are more accessible than in the southern hemisphere. To complete the work there must of necessity be a corresponding observatory in the northern hemisphere, and to complete the whole work in any reasonable time there should be several such pairs of observatories.

To make a beginning, so as to test the accuracy and probable value of the work thoroughly, it would be well to install one observatory of the kind in the most favorable situation and to confine the work, say, to four overlaping areas at each alternate hour of R. A. in each zone of 4° in declination and from declination 60° at each 4 hours of R. A. The results of such a series of pictures, taken and discussed, would lead to results of immense general importance, and would give some close approximation to the average parallax of stars of different magnitudes and proper motions, and would be an excellent pioneer program to ascertain the weak points of the original arrangements. It would not too greatly increase the program if plates having for their centers a number of the stars of more remarkable proper motion were added—indeed, perhaps the program might be best begun with these.

A, 4. I am hardly disposed to support the plan suggested by you of extending the zones on the plan of the *Astronomische Gesellschaft* from declination —32° to the south pole.

It is a far more accurate and useful plan to select the stars which are best distributed for determining the constants of photographic plates, as has been done at the Cape for declination —40° to —52°, and then to determine from the photographic plates the places of all required stars—say all stars to the eighth or ninth magnitude.

On the *Astronomische Gesellschaft* plan you get for this purpose an unnecessary number of stars in some parts of the sky and an insufficient number in others. For the zone —40° to —52° I found 8,000 stars ample, and they are as uniformly distributed as it is possible to select them. From the coördinates of our plates (and we are

about to publish zones —40° to —42°, the rest following in course of a few years) with the plate constants which will also be published any computer can construct a catalogue of all stars to the seventh, eighth, ninth, tenth, or eleventh magnitude, as he may see fit.

A, 5. The discovery and measurement of close double stars is an important branch of astrometry which is far behind in the southern hemisphere. I have great hopes that ere long a powerful telescope will be erected at Johannesburg for this purpose. Mr. Innes, recently my secretary, has been appointed in charge of the observatory there. At present his work is officially that of meteorologist, but I have great hopes that, having regard to his proficiency as a double-star observer, his enthusiasm and his power of exciting scientific sympathy, and the number of wealthy and large minded residents there, he will ere long be provided with a first class equatorial fitted for research on double stars. Meanwhile I propose to lend him the portable observatory I used at Ascension and a 6-inch equatorial of my own, which he is to employ in making an independent determination of the magnitudes of a number of stars on each of the C. P. D. plates.

But this prospect should in no way interfere with the erection of a second large telescope devoted to the same work, for independent comparison is at least as important in this department as in fundamental meridian work.

I had the satisfaction, when I visited Johannesburg last May, of selecting a site, outside of the town, which I have little hesitation in saying is one of the finest in the world for an observatory. It is nearly 6,000 feet above sea level, the atmospheric conditions seem to be most favorable, and on my recommendation the site, 10 acres in extent, has been secured by the government.

So much for astrometry. I agree with you that the provision made under the direction of Pickering at Arequipa and the labors of Roberts and Innes in South Africa sufficiently fulfill the requirements of photometric research as compared with that class of work in the northern hemisphere, and the work of the Carte du Ciel appears to be provided for.

Astrophysics.

B, 1. I have no hesitation in saying that what is required is the erection of the largest possible reflecting telescope for exact researches on the spectra and motion in the line of sight of the fainter

stars. What you want is an instrument that will collect the largest possible amount of light from a star within the jaws of a spectroscope slit.

Not only does a reflector provide this on a larger scale than it is possible to attain in a refractor, but it unites the rays of every refrangibility in one focus.

For spectroscopic work a Cassgrain reflector seems the best form to adopt, as the cone of rays, with its smaller angle of convergence to focus, permits use of a comparatively long collimator. The position of the spectroscope is also convenient, because, having regard to the weight of the speculum, the radius of motion of a spectroscope near the speculum end must be much smaller than that of one attached near the principal focus of the large mirror, and it is also much more convenient of access in the former than in the latter case.

Of course, photographs of nebulæ, &c., could be taken in the focus of the principal mirror, but this is a less urgent need than the spectroscopic researches.

With the modern ball bearings and electric-motor motions I see no difficulty in conveniently mounting a mirror of 6 or 8 feet diameter.

The erection of such an instrument in some very favorable position is, I think, the next great step that should be taken. We are in a position now, with refined and well studied apparatus, to attack the determination of motions in the line of sight of all the brighter stars ; but to get sufficient light to photograph the spectra of the fainter stars under such dispersion as will furnish reliable determination of motion in the line of sight requires a telescope of greater light grasp than I fear we shall ever get from a refracting telescope. For less money than the cost of a 40-inch refractor one could mount a reflecting telescope of twice that aperture—*i. e.*, of four times the light grasp—that would unite all the rays of light from a star in one focus.

I put this so far beyond all other demands of astrophysics that I make no further suggestion, and I do feel that steps should be taken to urge its fulfilment.

I believe that Bloemfontein, in the Orange River Colony, would be an ideal site for the erection of such an instrument.

[*From Dr. M. Nyrén, Wirkl. Staatsrath, Pulkova Observatory.*]

[Translation.]

PULKOVA, *August 14, 1903.*

The proposition for the observation of stars in the southern hemisphere, which you were so kind as to send me, interests me greatly; so much the more in reference to some points of the program, because we also here have for a long time cherished ideas tending in the same direction. As a small contribution to the realization of these ideas may be considered, the extension of the Pulkova Fundamental Catalogue to —30°, now attained by observations in Odessa. By numerous reference stars these observations are intimately joined with the Pulkova system. I send by this mail the program and the list of stars for this combined catalogue.

I quite agree with you that the first task for the observing station in the southern hemisphere should be to create a trustworthy fundamental catalogue from —20° to the south pole. In this catalogue the Auwers list of 480 stars, extended to the pole, should be included; but I would add thereto a number of stars, especially in those places where the stars of Auwers' catalogue seem to be brighter than may be desirable for fundamental stars, third magnitude and brighter. In this manner we should obtain a standard list of about 600 stars, in accordance with your proposition.

For the observation of the right ascension of these stars it would not be troublesome to find many places qualified for the purpose. With the declination it is doubtless more difficult; in this regard even the most southern portions of Australia are, as I think, too far from the poles. Besides, the observations made at the most southern observatory, Melbourne, are apparently affected by suspicious anomalies in refraction. In my judgment, the fundamental declinations should be observed on the continent of South America, as far as possible south of the —40° parallel, at a station where the ground to south and to north is as nearly similar as possible. Under favorable atmospheric conditions two observers could accomplish in two years a fundamental catalogue of 600 stars with 8 to 10 observations of each. For the great secondary catalogues, which demand a longer time, the observations, in case of need, could be made at another station, and also with another instrument.

As the number of fundamental determinations of the southern stars is rather small, the catalogue in question should necessarily

receive great weight. It is then, according to my opinion, the best economy of time and money to provide the observing station with equipment of the first rank. Your proposition that the same instrument should be employed alternately north and south of the equator will help us to eliminate many errors from the star places. Concerning the nature of the instrument, I should, for the declinations, in place of the meridian circle, prefer a vertical circle of moderate dimensions—5-inch aperture. The vertical circle seems to me, for a new observing station, preferable also from this point of view, that the consolidation of the instrumental piers, which for a meridian circle requires a long time, is for the vertical circle of no importance.

As corresponding in accuracy to the declinations determined in such manner, the right ascensions should also be observed with a transit instrument. This part of the work could without inconvenience be made at another station, at Cordoba, or in Australia.

For the observations of the stars of the secondary catalogues, the instrument described by you will certainly do good service.

I submit these, my ideas, to your judgment, and I should be glad if you find them worthy of any attention.

I have delayed the answer of your letter until Dr. Backlund's return from a journey in Germany.

[*From Director O. Backlund, of the Imperial Observatory, Pulkova.*]

[Translation.]

PULKOVA, *August 20, 1903.*

In order to make my answer to your valued communication more intelligible, I premise the following remarks:

Fundamental determinations of star positions stand in the first rank among the chief undertakings of the Pulkova Observatory. To this end W. Struve had the transit and vertical circle constructed, by means of which the positions of the so called Pulkova "Hauptsterne," 381 in number, have been determined in three series, namely, at the epochs of 1845, 1865, and 1885 (about). In the year 1894, in accordance with a plan by Nyrén, the program of the two instruments was enlarged by Bredichin so that about 1,000 additional stars of the fourth to seventh magnitudes should be determined for the epoch 1900, in general after the same program as for the "Hauptsterne." This series of observations is now completed. When I undertook the directorship, in

1895, I endeavored still further to extend the fundamental observations, and accordingly brought about the establishment of a branch observatory at Odessa (north latitude 46°) whose task it is to extend the limits of our fundamental observations to —30° of declination. In accordance with the experience at Pulkova during the course of sixty years, the transit and vertical circle were selected as the instruments best suited for making these observations. After thorough reflection I decided in favor of dimensions of these instruments smaller than those of the Pulkova instruments. Since stars fainter than seventh magnitude would not be observed, the aperture of both instruments was fixed at 4 inches and the focal length at 4½ feet. With dimensions so small the observations can be made far more conveniently, and with the vertical circle twice as rapidly as with the Pulkova instrument. The observations for a fundamental catalogue of the same extent as that of the Catalogue of the Pulkova "Hauptsterne" were begun on the third of February, 1901, and, so far as the number of observations is concerned, upon the same program as that of Pulkova. The work was completed exactly a year after beginning, while such a series of observations at Pulkova has never been completed hitherto within less than seven years. Of course, this rapidity of work was made possible not alone on account of the more convenient instrument, but also on account of the decidedly better climatic conditions. The observations are already reduced, and it proves that their accuracy comes up to that attained at Pulkova. Both at Pulkova and at Odessa Repsold's self registering micrometer is adopted. This autumn the observation of the Pulkova "Hauptsterne" is to be taken up for the fourth time, and for the epoch 1905. On this occasion the number of stars has been increased to 500. Simultaneously about 200 more southerly stars will be observed at Odessa, so that the resulting catalogue will contain about 700 stars independently determined between the North Pole and —30° of declination.

It will probably be clear, after this circumstantial description of our fundamental observations, that nowhere can the news of the purpose to institute fundamental observations of about 700 fundamental stars in the southern hemisphere awaken a higher interest than here in Pulkova. Indeed we are permitted to hope, in accordance with your grand project, that at no distant time the long desired and absolutely necessary fundamental observations will be extended over the entire sky, to aid in the further development of

our knowledge of motions in the planetary system and in the stellar field.

That the observations, especially those which concern the declinations, should be made, if possible, under a southerly latitude of at least —40°, I am in agreement with Nyrén. As to that which pertains to the planning of instruments, in consideration of the high degree of technical skill in the construction of instruments in America, it might appear venturesome to express decided opinions from here. I content myself with calling attention to the experience which we have gained here in Pulkova and in Odessa with the transit and vertical circle as to the determination of right ascension and declination with special instruments.

If, in addition to the fundamental determinations, the projected zone observations can also be secured, you will in that way earn the thanks of astronomers for all time.

There can be no doubt that the derivation of astronomical constants from observations in the southern hemisphere is a thing to be desired. For example, the very discordant values of the aberration constant, which the determinations secured at different observatories have recently shown, prove how necessary classic observations are. Latitude variations demand observations at places separated from one another as widely as possible. In accordance with an agreement with Potsdam, a series of observations for this purpose will be undertaken here in Pulkova, and with a zenith telescope of 5 inches aperture.

In regard to your remaining questions, so far as I may be permitted to judge, they are not less rationally proposed. You will be able to count upon the unanimous approval of the scientific world not less for these than for the others.

That the existing arrears in astronomical work in the southern hemisphere is felt as a drag at every step in every investigation now in progress cannot be denied. If on that account the investigation planned by you is realized, then a great drawback will be thereby removed; but the well known American energy affords a warrant that the carrying out of this plan will not be long delayed. A speedy realization of this project means an immense advance in science.

[*From Director E. Becker, of the University Observatory, Strassburg.*]

[Translation.]

STRASSBURG, *August 23, 1903.*

I take note of your letter of July 8 with the highest interest, and I fully assent to the opinions therein expressed. It is a fact not to be overlooked, and one originating in the distribution of observatories upon the earth, that our knowledge of the southern heavens is very much behind that of the northern hemisphere. So long as this inequality exists we must necessarily relinquish the idea of obtaining a satisfactory solution of many and indeed the most important cosmic problems. That this deficiency will be remedied within a time which we can now foresee by the establishment of new governmental observatories in the southern hemisphere is not to be expected, and an effort must be made—which would be greeted with the greatest joy in case of success—to induce private institutions to lend a helping hand. An observatory established in a favorable site, equipped with modern instruments, under skilled and energetic direction, with a staff of experienced observers and practiced computers not too small in number, in my judgment would be able to complete a work which would suffice to fill up the gaps in our knowledge that are now most deplored.

What tasks are deserving of the most prominent place on the program is a question which it is not entirely easy to answer from an objective point of view, and the answer would also depend upon the means that are available. On the whole, I am in sympathy with the arrangement set forth in your statement, and, for my part, would assign the preference to propositions 1, 3, 4, and 5, without underestimating the importance of the other tasks. As to 1, it appears to me worthy of consideration, whether upon grounds of economy, the undertaking ought not to be limited to stars of the sixth magnitude, or, in any case, to those of the sixth and one half, and whether the determination of position for stars fainter than the sixth or sixth and one half magnitude should not be assigned to the zone work, which, according to experience, is susceptible of producing very accurate results.

[*From Dr. Ralph Copeland, Astronomer Royal for Scotland.*]

EDINBURGH, *26th August, 1903.*

I have read your letter and inclosure of July 8 with deep interest. Regarding the various classes of special observations to be made in the southern hemisphere, included in your statement, it seems to me that classes 1, 4, and 3 are of the greatest immediate importance, and their urgency is probably in the above sequence, the first being the most important.

Class 2 I consider of relatively far less immediate interest, seeing that the results arrived at are by no means of the fundamental character of those obtained by classes 1, 4, and 3.

Respecting class 1, it is conceivable that the observations might advantageously be divided into two groups: *a*, the fundamental determinations of the places of 600 principal stars, together with the essential observations of the sun; *b*, the precise observation of the 5,400 remaining stars brighter than the seventh magnitude between —20° and the south pole.

Group a.—Possibly this work could be most satisfactorily accomplished by using two instruments—a vertical circle and a specially efficient transit instrument. But these instruments, and in particular the cells of the object glasses, should be made of mild steel, which has a coefficient of expansion differing but little from that of glass and much smaller than that of brass, hitherto so largely used in the construction of astronomical instruments of precision. With the lenses held by springs on the plan designed by Fraunhofer, it is probable that they would rest almost absolutely immovable in their cells in all positions of the instrument, and that their minute real movements would be directly related to changes of temperature.

For the vertical circle I would suggest the trial of a design that occurred simultaneously to the late Dr. Common and to myself. It consists in placing an object glass at each end of the tube in such manner that the focus of either object glass shall fall absolutely on the outer surface of the other. Spiders' webs are to be replaced by fine lines, engraved or etched on the outer surface of each object-glass (I have seen lines of this kind not appreciably inferior to the finest natural webs). This construction would permit of the "end for end" reversal of the vertical circle, as well as of the ordinary "right and left" reversal, thus eliminating flexure from every determination of double zenith distance. I pass over the obvious

details of the light swing-frames with their counterweights carrying the eye piece at each end of the telescope and readily turned aside when not in use. I feel assured that such an instrument in the hands of a skillful observer would yield zenith distances of an accuracy not yet attained. One of our first opticians assured me that by the use of twin discs of glass and alternate grinding there would be no difficulty in producing the requisite pair of object glasses.

The transit instrument should be reversible on every object. It should have a clean drawn cylindrical tube of mild steel, attached in the simplest efficient manner to the middle of the enlarged steel axis through which it passes. This kind of tube was suggested by Sir David Gill in conversation many years ago. The transit to be taken by means of a movable recording wire—say, 10 seconds in each position of the instrument—by the well known method used so largely by Professor Albrecht and his staff in Germany. By this method collimation error and inequality of pivots are at once eliminated, and, as it seems to me, the troublesome magnitude equation is practically evaded. Moreover, it is certain that the personal equation is confined within extremely narrow limits. As it would be necessary to observe the sun with this instrument, it would probably be desirable to use a reversing eye piece; but experience would doubtless soon show whether this is desirable or not.

Group b.—This work—the precise observation of the 5,400 remaining stars brighter than the seventh magnitude between —20° and the south pole—could probably be rapidly and efficiently accomplished by the use of a meridian circle, which I should like to see made of steel, with the graduations on gold, platinum, or on an alloy of gold and palladium; but by no means upon silver, which is so liable to tarnish and necessitate risky cleaning.

The observations in class 4 are well worth undertaking with the least practicable delay, as they will gain largely in value by every year that elapses after they are once secured. By carefully boxing in the circle of the instrument used and securing an efficient circulation of the air confined within the box, it is probable that the accuracy of this class of observations could be measurably increased. Class 2 seems to me to be the least important part of the proposed undertakings, the resulting stellar distances being apparently peculiarly mixed up with those of the few available comparison stars. But may we not hope, now that the displacement of the earth's axis of rotation with regard to the observatory can be taken into account, that the fundamental observations of class 5 may begin to indicate

the annual parallactic displacements of all the nearer stars relative to the bulk of the 600 stars under observation? In course of time it would then be necessary to take these parallaxes into account in determining the apparent places of the stars affected by them.

Class 6 evidently calls urgently for a large refractor of the most perfect kind, used at a station where the definition is of the very best. Possibly this work could be best carried on at the proposed high level astrophysical observatory, although I may mention that at Jamaica, in the end of 1882, I found the definition near the level of the sea exceedingly good on many nights. In that island it would be possible to find a perfectly salubrious station at an elevation of 4,000 to 5,000 feet, and quite probably equally favorable localities could be found in some of the more mountainous South Sea islands.

The general sidereal part of your scheme ought, as you say, to be carried out south of latitude —30°, but in the southern hemisphere the climate increases in severity at relatively moderate distances from the tropics much more rapidly than it does north of the equator; hence every care should be taken to avoid a site too far to the south. At the same time I most earnestly support your view that no pains should be spared in choosing a thoroughly salubrious climate; otherwise the most devoted members of the expedition will be just the most likely to fall victims to any error of judgment in this most important matter.

In conclusion, I would suggest placing your most important timekeeper in a partial vacuum—say, under nine tenths of the normal local atmospheric pressure. This is easily secured in a cast iron box with a three-quarter-inch glass face resting on a slip of rubber. A "quicksilver sleeve" permits of winding the clock twice in the week. A small syringe removes any slight leakage of air. We have found here that the air in the box must not be dried artificially, or the oil necessary to the clock work will decompose and the clock will stop.

A great improvement would be to add an outer case, the air in which, by a simple electric contrivance, could be kept at a uniform temperature slightly in excess of the highest temperature likely to occur naturally. Under these conditions, any well made clock ought to have a rate subject only to very minute changes.

[*From Dr. W. H. M. Christie, Astronomer Royal, Greenwich.*]

GREENWICH, *August 29, 1903.*

I fully agree with the Committee that a much larger provision for astronomical observations in the southern hemisphere at the present time is desirable, and that with this object a Southern Observatory of an expeditionary character for definite pieces of work which could be completed within a limited number of years should, if practicable, be established.

Taking the special observations regarded as important by the Committee:

1. The proposed fundamental determination of star positions would be of great value. With this should be combined observations of the sun and determination of position of the ecliptic.

Observations of the moon, as nearly continuous as possible *throughout the lunation*, for several years are very much needed for the improvement of the lunar tables in regard to terms of short period, and these might well be combined with the observations of fundamental stars.

Observations of the planet Mercury are also much wanted.

2 and 3. I fully agree as to the importance of these and the need for making provision for them.

4. This does not seem to me so much needed at the present time. The observation of reference stars for the plates of the astrographic catalogue now being carried out at Cordoba, the Cape, Sydney, and Melbourne largely covers the ground, and though the number of stars is less than on the plan of the *Astronomische Gessellschaft*, the place of a far greater number will be determined from the photographic plates with an accuracy greater than that of meridian observations.

There is, however, a gap in the southern zones for the astrographic catalogue, viz., zones —32° to —40°, the plates for which are being taken at the Perth Observatory (West Australia), but with little prospect of their being measured there or of the necessary reference stars being observed. Another zone, —17° to —32°, undertaken by Montevideo, is in even worse condition, the funds for providing the photographic telescope not having been granted as yet, though promised by the president of Uruguay.

In place of 4, I should prefer to substitute the completion of the zones of the astrographic catalogue by the taking and measurement of the plates and the observation of the reference stars for the zones

not otherwise provided for. This seems to me to be urgently needed, and the work could be completed in a limited number of years.

5 and 6. I quite agree as to the importance of these.

7, I presume, may be considered later.

In the matter of location, I would submit that New Zealand and Tasmania should be carefully considered. An observatory established in New Zealand would have a good chance of being taken up by the government, as in the case of Cordoba, after its specific pieces of work were completed, and it would undoubtedly give a great stimulus to astronomy in the colony.

[*From Professor J. C. Kapteyn, Director of the Astronomical Laboratory, Groningen, Holland.*]

VRIES (NEAR GRONINGEN), *August 31, 1903.*

In answer to the valued invitation of your Committee, I will unreservedly state my views, though they may seem somewhat radical on some points.

For evident reasons there cannot be the slightest doubt that a southern astronomical observatory can do much more for the promotion of astronomy than a northern one.

The works falling in the first line for cultivation at such an observatory I consider to be:

1. Determination of stellar parallax.
2. Fundamental determination of right ascension and declination.
3. Determination of radial velocities of the fixed stars.

There are, most certainly, several other works which urgently call for execution, but I think the three works mentioned must take precedence of all the others. Moreover the chances of these other works being undertaken elsewhere on a more or less sufficient scale seem to be somewhat better. So, for instance, the extension of the astrographic catalogue from declination —32° to —90° in your "confidential statement."

At the Cape the positions of the reference stars for the "Carte du Ciel" have been determined by the meridian circle. This being so, the positions which will be obtained by the measurement of the plates (as soon as they shall have been reduced to right ascension and declination) will make the want of an extension of the astrographic catalogue little felt for that zone.

It seems but reasonable to hope that the existing southern observatories (Cape, Melbourne, Cordoba) will cooperate to furnish the

same material for the reduction of the remaining parts of the southern sky. Therefore, though (with a view to the indefinite time which may still elapse before we get a complete catalogue of right ascension and declination for use from the "Carte du Ciel") I would most cordially rejoice in an extension of the astrographic catalogue, I would still place this work in the second rank of the works most urgently demanded at the present moment.

However this may be, I will restrict my remarks to the three works mentioned, which are the most important of all.

(1) *Determination of Parallax South of Declination —20°.*

As, in my view, there is at present no work so urgently demanded for the advancement of astronomy as the determination of parallax on an extensive scale, the equipment of the observatory for this purpose should be as complete as possible; for instance:

a. Two photographic telescopes, say of 40 cm. aperture and 6 meters focal length. They ought to give round images over a field of 2° diameter.

b. One telescope of 40 cm. aperture and a focal distance as great as is compatible with rigid mounting. A round field of, say, 80′ diameter or even somewhat less will be sufficient.

c. One transit instrument of 7 inches opening.

d. One heliometer of 7 inches.

Regular morning and evening observations demand two observers for each instrument. A small part only of the time of these observers would be taken up by the observations; the rest would be devoted to the measuring of the plates and the reduction of the observations.

To provide such an outfit and such a staff exclusively for the purpose of parallax determination may seem extravagant. I do not think so. The need of a better and more solid knowledge of stellar distance is so great that we should stick to some such plan as is involved in the above, even if it appeared that thereby the funds available would be exhausted. If something must be sacrificed, I think the instruments *c* and *d* could be best dispensed with, as it seems more likely that the observations to be made by these instruments will be taken up elsewhere.

The two telescopes *a* would serve for a photographic Durchmusterung for parallax. Elsewhere (Publications Astr. Lab. Groningen No. 1, pp. 87–98) I have explained at some length the feasibility and desirability of such a plan.

Notwithstanding the reasons adduced (pp. 97 and 98), some astronomers still think that we ought to restrict ourselves to the brighter stars and those of considerable proper motion.

We may now exclude the former from consideration, because, for obvious reasons, a photographic Durchmusterung will do little for the very brightest stars. They must be treated by instruments *c* and *d*, and there will be little difference of opinion as to the desirability of investigating as many of these stars as may be possible.

The real question thus is: Has the time come to make a complete Durchmusterung of parallax for the fainter stars (say 6.0 to 10.0)? Or may we restrict ourselves to stars of sensible proper motion only?

It will be a relatively small undertaking to obtain the parallaxes of the 200 stars of greatest proper motion (this is about the number of stars with proper motion exceeding 0″.6, known at present, a great part of which are bright ones).

It seems not too much to expect that these will be observed elsewhere. In fact, I think the greater part of them have already been measured. For the rest, if necessary, they will be dealt with by the instruments *b*, *c*, *d*.

Setting aside also the consideration of these stars, therefore, it remains to answer the question: What are we to do afterwards—after the observing of the 200 stars of greatest proper motion?

The more important part of our aim must be to get a knowledge of the distance of a great part at least of our nearest neighbors in the universe, in order that we may begin by making a study of the laws in their distribution and motions. Now, if from the very beginning we exclude all the stars of which the proper motion at right angles to the visual ray is small (and we virtually do this by confining ourselves to stars of great proper motion), then we may foresee at once that the finding out of any real law in the motions will be impossible. Our aim will be defeated from the very outset.

There are other considerations more amply set forth in the paper quoted which must lead to the conclusion that for a study of the construction of the stellar world we cannot escape the necessity of making a Durchmusterung for parallax. It would be a noble task for the observatory to be erected to take the lead in such an undertaking.

For a fairly fine climate I estimate that the two telescopes *a* together would furnish a duplicate set of plates for the whole sky from declination —20° to declination —90° in about eight to ten years.

All the objects found that are suspected to have a fairly large parallax would be taken up by telescope *c*. They would be further investigated only in those cases in which a first plate confirmed the large parallax.

On the plates furnished by this telescope only the principal object, with five or six well chosen comparison stars, would be measured. The work of measuring and reducing would be very moderate, therefore, especially as a reduction with three constants would be sufficient in nearly every case.

Instrument *c* would serve for the parallaxes of the brighter stars (say 0—6) and, together with instrument *d*, for the further investigation of objects of certainly measurable parallax.

The observatory ought further to be fitted out with (say) ten measuring machines. It is very probable that the observers, though their labors at the telescope would take up only a small fraction of their time, will be unable to make the measurement and reductions keep pace with the production of the photographs. It will be necessary, therefore, to procure assistance for them. This, however, will be a question of cheap labor. (See P. S.)

I have dwelt thus long on the subject because it is practically a new one. On the others I have only a few words to say.

(2) *Fundamental Determination of Right Ascension and Declination.*

The great importance of a fundamental determination of star positions, with extension of these observations by secondary methods to include every star brighter than the seventh magnitude south of —20°, is evident to any one who has made some study of stellar motion.

I feel very warmly for the plan of transporting to the southern hemisphere, for a short term of years, one of the reversible meridian instruments of the northern hemisphere. The observations of a considerable number of stars with the same instrument at a northern and at a southern observatory cannot but lead to a material reduction of the influence of systematic instrumental error and error of refraction.

It would seem to me that it would be advantageous to make the determinations of right ascension and declination by two separate instruments, a transit (for which the instrument also used for parallaxes may serve) and a vertical circle. Besides other well known reasons, there is this one: If for the determination of declination we bisect the image of a star by the horizontal wire, its

brightness is very considerably reduced, especially in the case of the somewhat fainter stars. Thereby the personal error depending on brightness must be changed and an element of uncertainty introduced into the right ascensions. If I am not mistaken, the effect was found to be quite perceptible in Leiden.

As the elimination of systematic error influenced by refraction and flexure is of such paramount importance for our fundamental declinations, I would suggest to supplement the above determinations by the determination of some hundreds of fundamental declinations by the method explained in Copernicus III, pages 147–182, which make the result absolutely free from both refraction and flexure. * * *

(3) *Determination of Radial Velocities.*

I have no suggestions to add to the plan developed in the "confidential statement," with which I most cordially agree, as far as I can judge of the matter.

P. S.—In regard to the measurement and reduction of the plates for a Parallax Durchmusterung, which might perhaps be considered to put too heavy a strain on a single observatory, I would like to add that no doubt many of the smaller observatories, not too well provided for, would be only too glad to do the work of first class importance by measuring and reducing good parallax plates. Under certain conditions the laboratory at Groningen would very willingly undertake the complete work of measuring reduction and description for a duplicate set of plates for a zone of 2° width at least.

[*From Dr. Arthur Auwers, Secretary of the Royal Prussian Academy, Berlin.*]

[In a private letter to the chairman Dr. Auwers discusses various topics. The following extracts refer to the plan for a proposed observing station in the southern hemisphere.]

[Translation.]

GREIFSWALD, *October 6, 1903.*

The "confidential statement" enclosed in your letter of July 7 designates the most pressing astronomical tasks to be worked out in the southern hemisphere so fully and, according to my judgment, so much to the point, that I find very little to add thereto; and, furthermore, my pressing work has hindered me until now from pre-

paring an answer. You wish me to express myself especially in regard to Nos. (1), (2), and (4), and, although belated, I will do this.

The *most important* task that today confronts the southern observatories is, in my judgment, the production of *really fundamental* determinations for a selected list of stars. Such determinations for the southern hemisphere are still wholly wanting. We are hoping for a series of such within the next few years from Gill, but it is of the highest importance that we shall be in possession of *several* such determinations, homogeneous and each as a check on the others, and the establishment of a temporary observing station for this purpose would therefore be a timely undertaking.

The employment of a meridian circle, which should be used before and afterwards for similar determinations in the northern hemisphere and which has proved itself to belong to the first rank of first class instruments, would be wholly worthy of commendation. By this means one would at least, to a great extent, remove one element of uncertainty, the adopted flexure. It is possible that the advantage from the employment of this device will not be so great as you apparently hope, since the principal source of uncertainty in our declinations arises from the uncertainty of the refractions, in which local anomalies remain, arising partly in the observatory and partly in its surroundings, and which can be rendered less and less harmful in their effects through increase in the number of observing stations.

The attempts which have hitherto been made to establish an absolute system of declinations through comparison of observations made in opposite hemispheres are founded on the supposition that the refractions on both sides of the zenith are alike, and I doubt whether this supposition is correct for the majority of those observatories upon whose observations we have had to rely up to the present time for the establishment of systems of declination. The correctness of this supposition seems to me especially doubtful in relation to the two southern observatories which, up to the present time, have afforded the most accurate places of the brighter stars. The Cape, as well as Melbourne, observatories have the ocean to the south and a heated continent to the north, and over these different regions there may be very differently arranged masses of air. I consider it, therefore, very important that the new observation station should not have a similar position, but that it should be either purely insular or purely continental. At the same time, the southern-

most latitude possible is desirable. Perhaps the southern part of the Argentine Republic offers what is, on the whole, the most practicable compromise (also the possibility of existence for several years).

As closely related to the fundamental determinations and as a work to be accomplished with the same instrument it is very desirable, as you propose, that there should be complete observation of all stars south of $-20°$ and to the seventh magnitude, inclusive, four times for each star (equally distributed in the two positions of the instrument, and preferably according to the example set by the Pulkova series for 1855, with exchange of objective and ocular). In accomplishing this the period of observation will be scarcely lengthened and a second work of the first importance would be produced.

Of still greater importance than this second work, attached to No. 1, is the continuation of the astrographic zones, the proper and speedy observation of all stars down to the ninth magnitude upon the southern sky (No. 4). In Cordoba this work would have been extended beyond $-32°$, but since Dr. Thome has now undertaken a part in the photographic chart it is very desirable that others should undertake the continuation of the zones. For this purpose, in addition to the meridian circle for No. 1, a second instrument would naturally be required; but I do not see the use of giving to this instrument a construction of the form you have in mind. The ordinary meridian circle is entirely suited for zone observations, and in any half way favorable climate with such an instrument two observers (one at the ocular and the other at the microscopes) could with ease make 10,000 observations annually at less than $-40°$ of latitude where one has sufficiently long nights at all seasons; two sets of observers could make 20,000 observations annually.

No. 2 is also a task of great importance. The determination of the mean distance of the stars of various orders of magnitude is necessary, in order to provide a firm foundation for investigations into the structure of the stellar universe. But it appears to me impossible to reach this result otherwise than when one investigates the parallax of each single star of the brighter orders of magnitude, together with a sufficient number of the following orders, as far down as the means of measurement will permit. Dr. De Ball has planned such an undertaking and is corresponding in relation to it with others who have heliometers at their disposal—among others with Dr. Elkin. Therefore I will not go into further particulars, and will only remark that it goes without saying that it would be

of the highest value to secure for this enterprise the cooperation of another southern observatory in addition to that of the Cape, this also to be provided with a 7 or 8-inch heliometer. In my opinion, this is the only instrument with which, up to the present time, one has been able to secure reliable determinations of parallax.

* * * * * * *

With these remarks I have desired to express my personal and lively interest in the plans of the Carnegie Institution, for which I wish a speedy and complete fulfilment. I leave to you to communicate to your fellow members of the Committee so much of this letter as you think best, but otherwise I desire that you will consider it a private answer to your communication.

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II. CORRESPONDENCE RELATING TO PROPOSED SOLAR OBSERVATORY.

Introduction.

In January, 1903, a confidential statement regarding a proposed solar observatory was sent by the Secretary of the Committee to a number of astronomers and physicists. This letter stated that the principal purposes of the observatory, as they then appeared to the Committee, would be to investigate (1) the intensity of the solar radiation and its possible changes during a sun spot period; (2) the problem of the solar constitution, through observations with the spectroscope and other instruments; and (3) various stellar and nebular problems connected with the evolution of the sun and stars. The necessity of choosing sites especially suited for such work was also pointed out, and the suggestion was made that for the study of the solar constant a high mountain station, with a second station near the base of the mountain, might be required. Suggestions were requested regarding the proposed program of work, the selection of sites, and any other subjects connected with the observatory.

In response to this letter the following replies were received:

Letters from Correspondents.

[*From Professor C. A. Young, Director of the Observatory, Princeton, N. J.*]

February 7, 1903.

Naturally I am very much interested in the question of a special astrophysical observatory. There is no question that the lines of

research indicated in your "confidential" paper are important and ought to be followed up in some concerted manner; but I own to some doubt whether it would be best that all that research should be concentrated at any one, two, or even three stations. Cooperation between workers widely enough separated to secure nearly continuous observation might be better, unless some locality can be found where observations are practicable *all the time*, and I know of no such locality, in the United States, at least. Still it is obvious that, given the "ideal" director with adequate means at his disposal, there would be great advantages in the concentration, perhaps quite sufficient to overbalance the disadvantages.

As regards my experience at Sherman, it did not indicate any advantages as to average aerial conditions over Hanover and Princeton, but during my six weeks' stay there (I think it was six weeks) there were two or three magnificent nights, when the conditions were better than I ever saw them here (one night here, perhaps, excepted, or rather a few hours that night). For solar observations, however, the conditions from half an hour after sunrise till 9 or 10 a. m. were fine more than half the time. About 11 it usually began to cloud up, and in the afternoon thunder storms were in order till 8 or 9 p. m., and for some hours afterward the air, though very transparent, was very unsteady. The seeing may have been good after midnight, but I did not examine it often, as my work on the sun gave my eyes all I could safely do with them.

Of course, my statement as to the behavior of the weather can not safely be taken as applicable to all years and months. It was in the months of July and August, 1872, that my observations were made, and I remember that some of the few residents of Sherman said that the conditions were unusual for those two months on account of the unusual amount of snowfall the preceding winter on the mountains west and south of Sherman; but from all I can learn I should think there was much more likelihood of finding better average seeing not very far from the sea, as in southern California.

I ought to add that undoubtedly an outburst of vigorous solar activity on the sun's limb from August 1 to 6 had a great deal to do with my success in finding new chromosphere lines. During the first three weeks of work I made little headway and was much discouraged. When I began to get up and go to work at 5 or 5.30 a. m. things went better, but the days I have mentioned, August 2–5, gave me fully half my harvest.

[*From Professor Henry Crew, Director of the Physical Laboratory of Northwestern University, Evanston, Illinois.*]

FEBRUARY 10, 1903.

I have not earlier replied to your circular letter of the 30th ultimo for the reason that I have nothing of value to contribute in the way of suggestions. Congratulations are certainly due to the astrophysical world on the splendid prospects set before it by the Carnegie Institution.

My own experience, my reason, and my reading all lead me to think that you are not likely to put too much emphasis upon the necessity of untying the bundle of sticks before you attempt to break them. By which I mean to say that you are hardly likely to find at any one station the best conditions for undertaking more than one of the three problems which you outline.

The best conditions for the solution of any of these questions would seem to me something like the following:

1. A carefully and intelligently selected site to which an investigator might go with confidence.

2. A single, definite, and not too general problem; at least, a single problem at a time.

3. The selection of two men whose interest and ability in the matter no one doubts.

4. A simple, plain, but adequate material equipment in all except the central and essential instrument, and then make this the most powerful and most efficient in existence.

5. Study the men you put in charge, see that they are comfortable, but not "too comfortable," and, above all else, see that the conditions (mechanician, etc.) are such that these men's time can all go to the problem in hand. In other words, leave your men "foot free," and then hold them responsible either for results or for difficulties which are certainly insurmountable.

6. Energy without haste; test men and sites deliberately.

Of all the problems which you mention, the most pressing appears to me to be the need of a continuous record of what is going on at the solar surface. Psychologists often find abnormal cases the most instructive. It may be so with solar studies. Next most important appears to me the horizontal telescope of large aperture. I think this deserves a fair trial in the best attainable spot on the globe, both for spectroscopic and for photographic work.

[*From Professor E. F. Nichols, Director of the Physical Laboratory, Dartmouth College, Hanover, N. H.*]

FEBRUARY 12, 1903.

Your letter of January 28, concerning plans and projected work for a new national observatory, has been received. The plans for work embodied under the three heads in the letter seem to me admirable, and to include work of the most valuable kind yet to be done in astrophysics. My own work in astrophysics has been very limited, as you know, but the outlook and far reaching extent of the projected work for the new observatory seems to me in its variety to include nearly everything at present worth doing.

In particular the conditions required for the most successful measurement of the heat radiation of the brighter stars would be a clear and quiet atmosphere for night work, and a large concave mirror of at least 5 feet aperture. The mirror must be so mounted that the beam from the mirror will be reflected in a fixed direction, so that the heat measuring instrument need not be moved in following the star. It is further very desirable, if not absolutely necessary, that the radiometer or other heat measuring instrument may be surrounded by constant temperature conditions during observations. The results which might be expected from such an equipment I have already discussed in my paper in the *Astrophysical Journal*, vol. 13, p. 138.

I shall be only too glad to do anything I can to further the plans of your committee, and only wish that any suggestions that I might make could be based on a broader experience in practical astrophysical work. If at any time I can be of use to your committee in any way, I hope you will have no hesitation in calling upon me.

[*From Sir William Huggins, Tulse Hill Observatory, London.*]

FEBRUARY 17, 1903.

I am very glad to hear that there is some prospect of establishing new observatories to provide for observations and researches which require special conditions of position or of equipment.

The lines of work sketched out in your letter appear to me to be admirably thought out, and indeed so complete from the point of view of the investigations which have the more immediate claims that I do not see that there remains much, or indeed anything, for me to suggest.

I think that it is of first importance to have a permanent observatory furnished with a large *reflecting* telescope and a complete equipment of auxiliary instruments for astrophysical research on some site with the most favorable conditions of atmosphere. If this were near the equator, it would command the richest regions of both hemispheres. I mention this point in case it might not be found possible to build a separate southern observatory.

The observatory next in importance, it seems to me, would be one on the top of Mount Whitney, devoted especially to solar work, or chiefly so. I think the photographic method of getting the corona should be tried. Theoretically it is certain of success, if only the atmospheric conditions are but a little better than normal surface ones.

I suppose work could only be carried on during the summer, but if the conditions are as good as the altitude would suggest, there is certainly work enough for many years to come.

The observatory at the base might be regarded as temporary, and perhaps might be given up when a sufficient number of observations simultaneous with similar observations at the observatory on the top had been made.

[*From Professor Arthur Schuster, Director of the Physical Laboratory, Owens College, Manchester, England.*]

FEBRUARY 18, 1903,

In answer to your letter of the 28th January: I should, of course, be highly pleased if funds were to become available for the important work sketched out by you. Taking the different points of your letter in order:

1. I have recently looked carefully over a good deal of literature concerning solar radiation, and I confess I have not been impressed by the probability that simultaneous observations at high and low altitudes will help us very much. The differences which such observations could show would all be due to the layer of air included between the two levels. On different days the atmospheric conditions of that layer may be very different, and yet the atmospheric conditions at greater heights than, say 15,000 or 20,000 feet, might be the same. You would at once have errors introduced, and the observations at the high altitudes might by themselves give you better results than the combination. You must face the fact that it is impossible altogether to eliminate changes in atmospheric condi-

tions. Supposing that you were to come to the conclusion that the solar constant observed, even at high altitudes, varies with a sun spot period, it would still be open to doubt whether the change is not due to something that happens in the higher layers of the atmosphere. That, in my opinion, would be the most probable explanation, but as even this fact would be important to establish, I quite agree that observations on the solar constant should be made at frequent intervals.

Whether there are any actual differences in solar radiations at different times is a question that will, I think, be solved in a different manner. It seems to me exceedingly unlikely that any increase or diminution in solar radiation can take place equally and simultaneously all over the solar surface. If sun spots have anything to do with it, we must imagine the changes to come out differently or at least at different times in different solar latitudes. I would therefore consider it a matter of first importance to improve observational methods as much as possible, so as to be able to compare with the utmost accuracy different parts of the solar surface in different portions of the solar spectrum. A change in temperature of even 100° ought to make an appreciable difference in the radiation at the violet end, though the radiation in the red will not be affected nearly as much. I have been very much struck with a recent paper by Mr. Wood, of Baltimore, which describes a screen that absorbs the visible light, leaving the ultra-violet. I should say that simultaneous photographs taken of the solar disc by the ordinary method and with this screen might give interesting results.

2. I think you know already the great importance I attach to the careful investigation of the spectrum of sun spots, and the other points you mention are of equal interest.

3. Here again I agree with you that such work as the measurement of the heat radiation of stars and investigations of spectra with very high dispersion are bound to lead to important results, and if your present atmospheric conditions at available observatories are not sufficiently good it would no doubt be highly advisable to have a site specially selected for the purpose.

I am not, of course, able to judge whether the same site may be suitable for the solar and stellar work, and I do not want to discourage altogether the possibility of a station at the base of a mountain on which solar observations are taken. Some useful results might be obtained in this way, but I should not hope for very much and should not personally be inclined to recommend any great ex-

penditure for such a purpose. Possibly a very moderate equipment at the base would do all that is desirable.

Might I also mention, in addition to the matters you speak about, that a repetition of Dunér's work on solar rotation in the reversing layer is, I think, called for? I do not think the last word has been said in that matter.

[*From Professor H. C. Vogel, Director of the Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

FEBRUARY 19, 1903.

In reply to your kind letter of January 15 in regard to the establishment of a great astrophysical observatory with stations at particularly favorable points, I wish to reply that I can only commend in the warmest way the realization of this great undertaking in the interests of science, and that I regard the investigations proposed by you, on the subjects named below, as so suitable and exhaustive that I am unable at present to suggest anything further:

(1) Solar radiation problems, involving the measurement of the solar constant at frequent intervals throughout the sun spot period.

(2) Solar investigations, principally of a spectroscopic nature, which require atmospheric conditions and instrumental facilities superior to those hitherto employed.

(3) Various stellar and nebular problems, such as could be undertaken to the best advantage with the aid of a large reflecting telescope.

In Europe we are not so fortunately situated as to be able to find so easily sites from which the proposed observations could be carried out to advantage. We can therefore only wish success to our American colleagues if the Carnegie Institution should provide the means necessary to carry out such comprehensive investigations, which are so important for astrophysics.

[*From Professor H. Kayser, Director of the Physical Institute, University of Bonn, Germany.*]

[Translation.]

FEBRUARY 20, 1903.

I am so busy at the end of our semester that I can reply only briefly to your letter of January 30.

If a new institution, provided with unusual instrumental means, is to be established for the study of the sun, it should have the solution of one especially important and fundamental problem as its principal purpose. The most important question seems to me to be with regard to the constitution of the sun. We seem to be infinitely far from the solution of this question—whether it is wholly gaseous or in part liquid or solid. This question must be solved through a very detailed study of the various parts of the sun. It seems to me particularly important to make a study of the sun spots. If their spectra could be photographed in the greatest possible detail throughout a solar cycle with large gratings, an important advance would probably be made. In this way it might be possible to form a proper estimate of Julius's theory—either to confirm or refute it. This investigation is a rather thankless task, since it consists only in gathering observational material; but a result drawn from it would be far more important for our knowledge of the heavenly bodies than the measurement of the radial velocity of a few hundred stars or the intensity curves of variables. As soon as the characteristic features of our sun are thoroughly understood, many other phenomena will explain themselves.

I would advise the use of plain gratings for this investigation, since freedom from astigmatism is necessary; the lenses should be preferably of quartz-fluorspar, and the solar image on the slit should be of large diameter.

Terrestrial spectra must, of course, be employed for the explanation of the phenomena. The laboratory must, therefore, be supplied with concave gratings of various numbers of lines to the inch and various radii of curvature; also with direct current for arc lamps and alternating current for transformers, as well as induction coils of various dimensions up to one meter spark length, in order that the spectra of the elements may be studied under the most varied conditions.

I have touched upon only a single question, which seems to me to surpass all others in importance. It goes without saying that a large institution would also undertake investigations requiring night observations. I name here briefly only two of investigations which seem to me particularly important: the spectra of the planets, in order to determine the nature of their atmospheres, and the spectra of a number of the brightest stars with a precision approaching as closely as possible that of Rowland's solar spectrum, so that their chemical constitution can be accurately compared, and if differences

are found in the spectrum of the same element the cause of these differences may be investigated.

I believe that the problems which I have named have hardly received serious consideration hitherto. All are for the purpose of determining accurately the physical and chemical constitution of at least a few of the heavenly bodies. In the case of the sun itself Rowland's atlas and tables may be regarded as only the first though a most important step toward a knowledge of its chemical constitution. A further study of the spectra of the elements would certainly permit 90 per cent of the lines now designated as unknown to be identified.

[*From Dr. W. E. Wilson, Private Observatory, Daramona, Ireland.*]

FEBRUARY 24, 1903.

I was very glad to receive your letter of January 28 and to see that there is the prospect of founding a large observatory for investigations on solar radiation and kindred subjects. I am sure such research can be profitably carried on only in situations such as you propose. Ireland is certainly *not* one for such work. I have been experimenting for two years with a new recording bolometer, which I think would give excellent results if it were mounted in a situation where it would get some sunshine. This it does not get here. If you would care to have it tried at your new observatory I would be delighted to send it to you. All you would require would be one of Callendar's electric recorders to work with it. It consists of two flat coils of platinum wire blacked and sealed up in an exhausted glass tube. This is enclosed in a brass tube with suitable diaphragm, so that one coil only receives radiation from the sun and a small bit of surrounding sky. The other coil is in the shade. These form the arms of the Wheatstone bridge of the recorder, and it gives a continuous record of the radiation. With the form of receiver designed by Callendar the coils were not in vacuum, but merely covered with a glass shade. One coil was black and the other bright, and both lay horizontally, so that the sun was never normal to them, and of course changing from hour to hour. I found that with this old receiver it was quite impossible to calibrate the curve with Ångström's pyrheliometer. Even on a dark, wet day it gave a considerable deflection, by reason of the glare from the clouds. With my new form the curve can be calibrated with the Ångström instrument perfectly, and by means of a planimeter, which is attached to the pen of the re-

corder, you can read every day the area of the resulting curve. I enclose you some sample records. Those marked A are taken with the old receiver and B with the new one. You will see that in B when there is no sunshine the curve falls to the zero line. In A it is always above it.

I wish you could also see your way to carry on a series of observations by allowing an image of the sun to transit over the aperture of a small radiomicrometer and recording the deflection on a moving photographic plate. By a discussion of the resulting curves I think it would be quite possible to determine whether the depth of the absorbing layer on the sun varies during a sun spot cycle. I began taking curves here, but the weather was so hopelessly unfavorable that I gave it up.

[*From Sir Norman Lockyer, Solar Physics Observatory, South Kensington, London.*]

FEBRUARY 25, 1903.

In response to your letter of January 29 regarding the work to be done by a new astrophysical observatory, I would say that your scheme practically covers the whole ground. There are, however, one or two points to which attention might be drawn:

1. Would it be advisable to erect a permanent building on a mountain summit without first putting up a temporary building and making observations from it for a year or two?

2. It is of great importance that the observatory should not be too far away from some large town easy of access, as modern work requires the investigator to be in touch and personal contact with scientific men for purposes of mutual assistance and advice in addition to the reasons given in your letter.

3. It is important that the laboratory equipment should be complete, for it is the mutual work of the observatory and the laboratory which helps the investigation.

4. The Janssen-Hale-Deslandres method of photographing the solar prominences should undoubtedly be undertaken and made strict routine work.

I am very glad to see that you mention the photographing of the ultra-violet end of stellar spectra, as this is important and we are at work upon it, although we have very small means here for carrying it on.

[*From Professor C. E. Mendenhall, Physical Laboratory, University of Wisconsin.*]

FEBRUARY 27, 1903.

Your letter of February 2 came duly to hand, and I have made a few notes concerning that part of the proposed work with which I am somewhat familiar. These I am very glad to send you now, hoping that the delay has been of no inconvenience to you. I have not thought that you wanted great detail, and have tried to avoid it. Though most if not all of the suggestions are such as must have already occurred to you, I nevertheless give them, thinking that perhaps they may usefully serve to confirm if not to initiate.

As regards the general scope of the work, I shall not presume to speak further than to emphasize the importance of one point which you mention, namely, provision for the study of such laboratory problems as seem intimately connected with the solar and stellar work.

In deciding upon the site it may be well to keep in mind, besides the primary requisites, the fact that the observatory will demand a constant though small supply of power. Possibly in California some long-distance transmission line or water power could be drawn upon.

For the solar constant work examples of all the best forms of pyrheliometers and actinometers should be provided and studied with a view to improvement. No one of them seems independently reliable at present, though the Ångström compensation pyrheliometer promises best.

For detailed infra-red spectroscopic work there is no doubt that the bolometer is the most immediately applicable, because of the work of Abbot and others at the Smithsonian Astrophysical Observatory. But the radiometer, thermopile, and radiomicrometer each has its peculiar advantages, so that if they were properly modified for linear spectroscopic work they might be used with advantage at the two substations. It seems to me unlikely that they will ever supplant the bolometer for accurate linear work in cases where the best must be had at any cost.

In designing the spectrobolographic outfit it seems to me questionable whether it should be as large as that of the Smithsonian Institution. At any rate, even if one outfit of such size is provided for the main station, it would be desirable to provide another of considerably smaller size, more or less self contained and capable of being used with advantage by a single observer. For some of the work

this could be used in place of the larger outfit with resulting saving in time and labor, and for much of the preliminary work and for the laboratory investigations it would be decidedly more convenient.

A storage battery equipment will be a necessity, small and portable, for the substations, but of considerable capacity for the main observatory. Especially should this be the case if laboratory work is undertaken, as this will undoubtedly involve the production of high temperatures which can best be done electrically. In this case also accurate means of measuring high temperatures will be necessary and a variety of laboratory sources of radiation should be available.

Of course, work would be greatly facilitated by a generous assortment of miscellaneous laboratory apparatus, such as small spectroscopes, telescopes, mirrors, polarizing apparatus, air-pumps, etc., and a lot of laboratory supports and attachments. Finally, as much of a shop equipment as is possible.

In connection with the spectrobolographic work, I must confess that at present the further detailed study of the infra-red solar lines does not seem to promise very much. Undoubtedly the true solar lines can be separated from those of terrestrial origin and identified to a greater or less extent with emissions of known elements, and this would perhaps be of most value in connection with the question of the persistence of given emissions through long ranges of temperature. Again, if the sun's surface were studied in greater detail the infra-red lines might help in the study of motions in the solar atmosphere, but it does not seem likely that they would be more important for that purpose than the lines photographically observable. Of course, it *may* be found that the infra-red lines behave in some ways quite differently from those of shorter wave length, and hence furnish a valuable tool for solar investigation, but it does not seem to me that the work so far done leads one to expect this. However, you know much better than I what to expect. Again, is it not true that the solar constant work and the separation of true solar lines from those of terrestrial origin are the parts of the work which really demand the high and low mountain stations, while much, if not all, of the other work, on account of its more intimate connection with laboratory experimentation, could better be carried on at a more centrally located observatory; for example, the Yerkes, best of any.

[*From Professor A. Riccò, Director of the Royal Observatories of Catania and Etna, Sicily.*]

[Translation.]

FEBRUARY 28, 1903.

With regard to the project under consideration by the Committee of the Carnegie Institution, of which you are a member, it is certain that if a station at the summit of a very high mountain is needed for the study of the solar radiation, another station will also be required which may be conducted without encountering the difficulties which are unavoidable at elevated stations; among others, the frequency of days when the mountain is enveloped with clouds due to the condensation which it produces. Etna, for example, as seen from Catania, is enveloped with clouds 167 days in the year, on an average.

The other station should not be placed at the foot of the mountain, since it would also experience the effect of condensations caused by the mountain. At Catania the clouds cover on an average only 39 per cent of the sky (Palermo, 46 per cent), but this amount of cloudiness would be still smaller if it were not for Etna, since the clouds appear more often on the side toward Etna (north) than elsewhere. Furthermore, the station at the base of the mountain would not have an entirely free horizon in one direction.

There should also be a third station on a very extensive high plateau on a small island, in order to have a very homogeneous atmosphere in perfect equilibrium, and to avoid disturbances of the images caused by ascending currents; these are very pronounced during the hotter part of the day on Etna, and carry up with them visible vapors, which frequently hide the sun, and invisible vapors which produce remarkable absorption of the solar radiations. On Etna the curves of heat received by the Arago actinometer have this form [figure not reproduced here]. This may be explained by the absorbing action of the ascending vapors during the hours of greatest heat. At Catania the curves have the regular form, rising with the altitude of the sun.

At Catania, as at Palermo, the images of the sun are best in the early morning; ordinarily during the hot seasons they are bad at 10 o'clock; they become better again before sunset.

[*From Professor A. Belopolsky, Imperial Observatory, Pulkova, Russia.*]

[Translation.]

MARCH 11, 1903.

The project of constructing several astrophysical observatories in the United States is of very great importance. Investigations of the sun are precisely those which require study by modern methods. At the present time, since the discovery which you have made with the spectroheliograph, the study of the solar surface with instruments of sufficient size promises to reconstruct current ideas regarding the constitution of the sun. The glory of accomplishing this will belong to the United States if the construction of special observatories is provided for.

It will perhaps be possible to find a mountain more advantageous than Mount Whitney for investigations of the solar radiation. I believe that the station can be established only at a distance of 300 meters below the summit. The plan for the investigations on the constitution and radial motion of stars also requires instruments more powerful than those which are employed at the present time. Everyone desires to undertake such work, but no country can realize this desire, since nowhere are sufficient funds available for scientific researches. It is only in the United States that private fortunes are devoted to science.

But, as you are well aware, successful investigators are quite as necessary as instruments. I believe that as many scientific investigators as the newly established observatories will need may also be found in America.

[*From Professor Cleveland Abbe, U. S. Weather Bureau, Washington.*]

APRIL 3, 1903.

Your letter of March 25, as Secretary of the Commission relative to the establishment of a large astrophysical observatory, interests me very much. The practical question as to the location and maintenance demands first consideration. There is no doubt that an observatory at the highest practicable point, working in cooperation with one lower down, will eventually add much to our knowledge of the solar and the terrestrial atmospheres. I consider Mount Whitney the most desirable summit station; a station at its base is necessary both for supplies and for special work on absorption. Another station in nearly the same meridian but farther south can

scarcely be found, but in place of that I think that the location of the Flagstaff observatory would be an admirable substitute. Among the solar investigations, I hope an effort will be made to get at the differences in radiation from different spots on the sun's surface in their successive rotations, so as to get the chronological variations in temperature, as well as the geographical differences. The complete course of work that you have sketched out covers all the problems that have thus far been found worthy of study, and, of course, the observatory will take up new ones as fast as they develop.

I should not encourage duplicating in the southern hemisphere all the investigations that are to be conducted in the northern, but there are some problems that could advantageously be studied at both observatories. On account of our knowledge of atmospheric conditions at the stations Charcani and El Misti, that location has some advantages, but there is still hope of finding an equally advantageous location on the mountains of Ecuador or southern Colombia. When such a station is found, it should be devoted especially to studies on nebulæ and stars, such as are described in your article 3, and perhaps also to less extensive studies on the determination of the solar radiation constant.

From a meteorological point of view, observations on these northern and southern mountain stations are extremely desirable, and especially if observers at the upper and lower stations can make absolute determinations of the altitudes and motions of the clouds or temperatures of the upper air by means of kite and balloon ascensions. Many other studies into the physics of the atmosphere, such as its gaseous constituents, its dust, and its motions, would constitute valuable additions to our meteorological knowledge. The special field to be occupied by such an observatory relates to the highest attainable atmospheric strata.

If there is any specific matter on which I can be of use to your Committee, I shall always be happy to respond.

[*From Professor G. Müller, Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

APRIL 4, 1903.

The plan to establish a great astrophysical observatory at a particularly favorable site, and to provide it with the best instrumental equipment, will be greeted everywhere with lively interest. For

many problems of astrophysics the atmospheric conditions experienced at most of our observatories are to be regarded as unfavorable. Every increase in the transparency and also in the steadiness of the air marks an advance, and for this reason the choice of a site for a new astrophysical observatory should be made with the greatest possible care.

The best conditions of the atmosphere are to be expected at a mountain station, and, according to my experience, an isolated peak is to be preferred to a point within a great mountainous region, where surrounding peaks under certain circumstances exercise a strong influence on the state of the atmosphere.

The higher the chosen point the better, but height is not the only important factor. It is far more important that the observatory shall not be too difficult of access, and, before all else, it is essentia that the observations can be made with as great convenience andl ease as at any other observatory. Occasional observations, such as are made during a very short period on a very high mountain under the most difficult conditions, are ordinarily of comparatively little value. Many problems of astrophysics, such as the determination of solar radiation and investigations on the absorption of the atmosphere, etc., cannot be solved during short expeditions, lasting days or weeks; they demand systematic study during a long period of time under the most varied atmospheric conditions—if possible, simultaneously from a peak and from a valley station.

In my opinion, it would be best to establish the proposed principal observatory as high as possible, perhaps at an altitude of about 8,000 feet, but in any event so that it could be kept in operation throughout the entire year, or at least through the greater part of it, and be at all times accessible without too great difficulty. At this place the principal instruments should be established, and all investigations conducted which relate to the spectroscopy and the photometry of the fixed stars, particularly those in which photography is employed. An important requirement is the provision of a second *permanent* station at a height of about 1,000 feet. This should likewise be provided with the best instrumental equipment, with the object in view to provide for certain observations which should be made simultaneously at both stations. It is naturally desirable that the direct distance between the two stations should be as small as possible, and that they should be connected with each other by telegraph and telephone.

As regards the program of investigations prepared by your Com-

mittee, I think that this covers all fields of astrophysics in an exhaustive way, and that hardly anything of importance can be added to it. I beg briefly to call attention to a few special investigations which, in my opinion, deserve special consideration :

(1) Investigation of the atmospheric lines of the solar spectrum by simultaneous observations at both stations under the most diverse atmospheric conditions and at all times of the year.

(2) Determination of the extinction of starlight by simultaneous photometric observations at both stations.

(3) Thorough investigation of the photographic extinction by simultaneous photographs of given groups of stars.

(4) Determination of the absorption for various regions of the solar spectrum by spectro-photometric measures.

(5) Comparison of the light of the sun with that of the moon, the planets, and the fixed stars, to determine by continued observations whether any variation in the intensity of sunlight can be detected.

In conclusion, I heartily wish you success in the great undertaking, and trust that your plans will soon develop in the interests of our science.

[*From Professor J. Hartmann, Royal Astrophysical Observatory, Potsdam, Germany.*]

[Translation.]

APRIL 12, 1903.

Let me extend to you my heartiest congratulations on the astonishing advances which astrophysics has already made in America. It must be acknowledged without envy that the new continent has wholly outstripped the old one, and I would regard the establishment of a high altitude observatory as a glory to American science. When, as here in Potsdam, one is forced to observe with a great refractor under very bad atmospheric conditions, one soon comes to appreciate the enormous advantages of a high station with transparent air and quiet images. I will mention here one point in particular regarding which I have had some experience. If a telescope is to be used for spectrographic investigations it is necessary to have the greatest possible aperture in order to secure great light grasping power. If the ordinary ratio, about 1 : 18, of aperture to focal length is employed, this large aperture corresponds to a very great focal length, and in consequence of this the images become so bad with unsteady air that the advantage of the great aperture is almost

wholly lost. In this way, *in an unsteady atmosphere*, our photographic refractor of 32 centimeters aperture gives practically the same results as the great refractor of 80 centimeters aperture. If a greater ratio of aperture to focal length is chosen, say 1 : 10—quite apart from the great thickness of glass required in the refractor—new difficulties result, in that the aperture of the collimator can be only 1 : 10, and consequently for a given size of prism the collimator will be too short.

At a high station these several difficulties are not to be feared. If you find a station with very quiet images, I should recommend for spectrographic work—either with very high dispersion on the brightest stars, *e. g.*, for the determination of the solar parallax, or with smaller dispersion on faint nebulæ and stars—a reflector with great aperture, and a ratio of aperture to focal length of about 1 : 30 or still less. I should also choose such a construction as to permit the spectrograph to remain in a constant position, with the collimator horizontal or in the direction of the earth's axis. Such a spectrograph with long collimator, very short camera, and very high dispersion would be best adapted to carry out the very important investigation on the motions within nebulæ to which I recently called attention. Our apparatus here did not permit me to accomplish much in this direction. If the spectrograph were built in a fixed position, not suspended from the eye end of the telescope tube, it could be made much more stable and also easily maintained at a constant temperature. It would thus be possible to employ very long exposure times. A horizontal mounting is also to be recommended for spectroheliographs of the largest dimensions. It is important to make these photographs with a very large solar image, in order, for example, to be able to study with precision the motions within a sun spot. Spectrographic studies of the zodiacal light and the aurora should also be included in the program.

I heartily wish success to the great undertaking, and it will give me pleasure if I can aid it in any possible way.

[*From E. Walter Maunder, Esq., Royal Observatory, Greenwich.*]

MAY 14, 1903.

With regard to the question of astrophysical research, my own position has led my thoughts in two directions.

To me sun spots seem to be the most important subjects of study. Our work at Greenwich consists, as you well know, in taking two

photographs of the sun daily on a scale of 1 decimeter to the solar radius and measuring one of these for the area and position of the spots. So far I think we fulfill our purpose sufficiently well. A larger scale is not necessary for positions of the accuracy we seek; indeed, the scale of 1 decimeter to the solar diameter was sufficient for that purpose and was less costly and the photographs were more manageable in size. To attempt to push the work of measurement and reduction to a further refinement would immensely increase the cost, and I doubt whether it would repay the outlay and trouble.

But when we come to the question of the details of the spot forms and of their changes, then this scale is certainly not adequate. I greatly wish, and have done so for years, that we had here a second telescope with which we could take comparatively small areas of the solar surface on a scale of at least 1 meter to the solar radius. I think this is needed to supplement the other.

But the study of spot spectra is much more urgently needed. My own slight experience of it with a most hopelessly inadequate instrument was sufficient to make me feel that it was absolutely one of the most important lines of research.

I have always felt it a great misfortune that Sir Norman Lockyer, in the work which he has carried on at South Kensington for so many years, should have devised the method of recording just the "twelve most widened lines." It seems to me in every way a badly devised scheme. If a long series of observations are conducted by one and the same observer, I should think it ran a great chance of stereotyping more or less accidental impressions. If the observer is often changed, we have no longer any means for comparing observations made at different times; and, at best, if we assume the observations free from all personality and absolutely immaculate in quality, they seem to me to tell us hardly anything at all. The general character of any particular spectrum—not to speak of important details—is left absolutely without record in such a system.

Sir Norman Lockyer's chief result, namely, that the most widened lines change with the progress of the spot cycle, opens out a great number of questions. First, the spots at maximum are not only more numerous, but they run much larger than at minimum. It would be most important to observe both at minimum and at maximum a series of spots of a definite size. I would suggest, as it is a size sufficiently frequent even at minimum, a spot of 200 to 300 millionths in area. Clearly it is a great assumption, if we find a

certain spectrum given us by a spot of area 200 at minimum and another spectrum given us by one of 2,000 at maximum, to ascribe the change to a quality in the period (if I may so express it) when it may be a function of the size of the spot itself. Further, the average spot group goes through a certain pretty well defined routine in the course of its growth and decay. Now, if we are at the minimum of the cycle, our groups run small, and it is only (in most instances) during one particular phase of its development that a group is likely to be a tempting object for spectroscopic examination. At maximum we may have plenty of giant groups, which can easily be followed spectroscopically during their whole career. Here, again, is a point which wants to be followed out. If we record a given spectrum for a certain spot, we have not learnt all that we can unless we trace the history of that spot back to its rise and onward to its disappearance, and determine at what particular stage of its development the observation was made.

We want to know whether we can associate different spectroscopic appearances with—

(1) The size of the spot; since the larger spots may be assumed on the average to be the deeper.

(2) The stage of its development. The depth may alter with the age.

(3) The changes that are going on in the group.

(4) The progress of the general solar cycle.

(5) The type of the spot group.

I have put the type last, out of its logical order, because it has seemed to me that, though the great majority of spot groups conform to one general type of evolution, yet occasionally we get spots of a very distinguishable form, and it is these spots, when of immense size, and not spots of the normal type, that are clearly and unmistakably associated with magnetic storms. It would be no small matter if we found that such spots exhibited some distinct spectroscopic peculiarity.

As to the method of observation, clearly the photographic registration of spot spectra should be the routine one; but it certainly should not exclude the direct visual work. Just precisely as our daily photographs of the sun's surface at Greenwich, however admirable for their purpose, leave us without any record of the processes of rapid change, so it would be with the photographic registration of sun spot spectra. They cannot possibly render direct work unnecessary.

Might I quote from a letter to me from Mr. Evershed on this subject. He writes:

"We are apt to become too familiar with sun spots to be surprised at their occurrence; but I am sure that when their origin and meaning is really understood, a key will be found to a great many other solar and stellar problems. I consider that as a preliminary the spot spectrum should be investigated with a bolometer in the visible region and infra-red to find out whether the discrepancy between thermal and visual estimates of spot darkness is real, and, if so, where in the spectrum is the excess of radiation measured thermally.

"Another point to clear up is the resolution of parts of the spot band into lines observed by Young and Dunér. Does this apply to all spots and to all parts of the spectrum, and is the emission spectrum of the photosphere itself really continuous under high dispersion?

"It seems to me that until the fundamental radiation of spots is satisfactorily cleared up the study of widened lines is of secondary importance."

The other subject to which my attention has been directed is the study of Jupiter. It seems to me that that planet ought not to be left to the scrutiny of amateurs, but should be systematically observed at some permanent and endowed observatory. As the sun is the only hot star which we can study in detail, Jupiter is the only cold star, and we are fortunate in having representatives of both ends of the series within our reach. In my connection with the British Astronomical Association, my attention has been drawn to the special field for work which this planet offers. The Association has done what it could in the matter. Its object was the training of amateur observers, and their direction to real systematic purposeful work in place of the desultory star gazing which is too often all that amateurs achieve. So far it has been most successful, and the Jupiter Section numbers quite half a dozen observers of the very first rank, beside others who may in course of time attain the same skill. But an association such as ours can never be a substitute for a permanent observatory. A very large proportion of its efforts must be spent in the work of training; there is no guarantee that any of its observers will be able to follow up a research for a long-continued period, and the means for the proper discussion of observations are quite lacking. I might mention, as an example, one of our most active students of Jupiter, Capt. P. B. Molesworth, R. E., of Ceylon. He has been working there for nearly eight years, and in a single apparition has obtained nearly 4,000 transits of spots

across the central meridian of Jupiter, revealing not a few interesting relations; but necessarily he will not be able to remain much longer at that station, nor can he give to his observations the full discussion they deserve. If work similar to his could be undertaken by a professional astronomer, who would have the time to fully discuss his results, at a permanent observatory, which would secure continuity to the work, I think it would ere long lead to our understanding the condition of Jupiter far better than we can at present. Obviously an equatorial or at least a tropical site for such an observatory would, as Captain Molesworth has found, have great advantages.

[*From Professor Knut Ångström, Royal University, Upsala, Sweden.*]

MAY 16, 1903.

I beg you to excuse my long delay in answering your very interesting communication on the astrophysical observatory planned by the Carnegie Institution, but a great many duties have hitherto made it impossible for me to reply. I hope, however, that you will not see in that delay a proof of indifference regarding a question that in fact interests me profoundly.

Unfortunately I can give no information as to the site of the observatory, having no personal experience with regard to the atmospheric conditions in the mountains of California. The establishment of two corresponding observatories at different heights will certainly be most valuable for the scientific results. As to the choice of a place, it will probably be of great importance to study the local conditions. My experience, derived from visits to Teneriffe, is that on the northern side of the mountain it is almost impossible to get satisfactory results in solar observations, while the southern side is very favorable for that purpose.

As to the instruments for measuring the total radiation of the sun, I am sure that the compensation pyrheliometer is at present the only convenient instrument, and I am willing to superintend the construction of the instruments that the observatory may think proper to order from the mechanician Rose in Upsala.

Probably the program of the observatory comprises also the registration of spectral energy by means of the instrument of Professor Langley. I may, however, call your attention to the advantage of making the registration also with another instrument, with less dispersion, which in a shorter time could give a general view of the

solar spectrum and of its changes. I send you an account of a simple arrangement for that purpose.

It would be of special interest if these observations could be combined with researches on the amount of humidity in the free atmosphere (by means of kites). I hope to return later to certain questions that I believe to be of great importance. It will always be a great pleasure to me if I can be of service to you.

[*From H. F. Newall, Esq., The Observatory, Cambridge, England.*]

CAMBRIDGE, *May 22, 1903.*

I almost hesitate to put down some of the ideas that have occurred to me about the establishment of a large astrophysical observatory, for they are rather crude, incomplete beginnings than formed judgments, and if I put them down at all it is only in the hope that they may help to clear matters rather than with the idea that they can carry any weight.

First of all, let me say it seems a grand project to provide for an observatory for observations of *secular* physical phenomena of solar origin. It is perhaps an open question whether it is desirable to provide for allied stellar studies. Many people must share the same instinctive doubt about a universal observatory as about a universal instrument. One may definitely count on individual enterprise to provide for many of the researches indicated in your paragraph (3). Existing observatories do or can or should deal with most of the studies referred to, and it would be a pity in any way to risk cramping either performance or obligation in these matters. Moreover, there are the elements of competition; as, for instance, in determination of motion in the line of sight for *fainter* stars one may count on advance from existing observatories.

If in your large astrophysical observatory large special apparatus were available it might well be desirable to let the place become, as it were, a court of final appeal, whither perhaps rival pioneers might themselves resort to put their views to the test. In such cases, for instance, it might be a matter of "more light" being needed, and the pioneers might be expected to take with them their own eye end apparatus for attachment to a large light grasping instrument; but one would like to provide in every way against retarding small private enterprise and pioneering elsewhere. One must avoid anything that would lead to the position, "Oh, it is no use doing this or that; they have that on their program up there."

Where natural competition is absent or periods of phenomena are long, there is a grand opening for a powerful combined attack. One may count, for instance, on special studies of solar rotation at existing observatories, simply because the period is short, but probably the variation of such short period phenomena in the eleven year period could only be properly tackled in an observatory where the outsetting aim was the study of secular phenomena.

Hence it seems to me that secular phenomena are the special province of such an observatory as you are contemplating; and it would seem a wiser course to concentrate attention on such observations as would have direct bearing on these, and to provide for a systematic discussion of observations already accumulated, as well as for a systematic study of phenomena in process of being observed, than to scatter forces on the study of many stellar problems.

In many ways probably more advance could be made by enabling a single observer to carry out his observations in several stations successively. The solar radiation "constant" is an instance in point. Considerations of this sort would lead me to think that in some ways it might be better policy not to lock up huge capital in one fine observatory outfit, but rather to help individual researches by providing means for having them carried on with, say, one or two complete outfits that could be moved to various points of the globe.

As I say, I hesitate to commit these remarks to paper. I suspect you are far beyond the elementary stage that these remarks refer to.

As to aims and researches, your program is a large one already. It is not clear to me why it should not include a new attack on magnetic disturbances, and possibly on atmospheric and electrical phenomena.

[*From Dr. Ralph Copeland, Astronomer Royal, Royal Observatory, Edinburgh.*]

AUGUST 15, 1903.

I am afraid you will think me remiss in only now replying to your letter of March 26; but I have indeed most carefully thought over your project and looked up my old papers on mountain observatories. I have not much to add to the views which I expressed in my paper on the subject in volume III of *Copernicus*, which you have; but, when consulting it, kindly substitute on page 230, line 22, 1.32 inch for 0.7 inch.

Another note on my South American trip, written for the British

Association Report for 1883, may interest you, and I therefore inclose a couple of copies.

My own impression is that to reap the full benefit of a mountain station one should aim at a height of fully 11,000 feet, and if well within the tropics an elevation of 12,500 feet might be occupied throughout the year without serious discomfort. Such an altitude in either temperate zone, however, would expose the observers to the most terrible weather and great hardships in the winter—*e. g.*, the floor of the Crater of Elevation of Teneriffe (7,200 feet, latitude 28¼°), according to the late Professor C. Piazzi Smyth, is swept by violent snowstorms every winter. The experience of residents and travelers in your own mountains will furnish you with abundant further examples.

My experience at Puno on Lake Titicaca (12,500 feet), in latitude 15° 50′ south, proves that observations can there be carried on under favorable conditions of weather and temperature at all seasons of the year. Indeed, the sky, on the whole, is much clearer in the winter, and therefore better suited for observations in general, although there are doubtless certain solar investigations which could in that latitude be better prosecuted at a season when the sun passes within a few degrees of the zenith for many weeks in succession. In the months of October, November, and December the weather is often very fine, I was told; but in January, February, and the early part of March clouds, and even a good deal of rain, are to be expected.

As you will know from *Copernicus*, my experience in the Andes was confined to the neighborhood of the Mollendo-Puno route, where, through the courtesy of the railway authorities, mechanical and technical aid is readily procurable; but doubtless the same mechanical facilities would be offered on the Oroya railway, which, starting from Lima, in 12° south latitude, reaches a height of fully 15,600 feet quite near the Pacific seaboard. Unfortunately the disturbed political state of the country at the time of my visit prevented me from examining this railway, but from its position so near the rainless coast it is very possible that the weather conditions near the upper part of the route may be fully more favorable than on the Mollendo railway.

But I should here like to draw your attention to a point affecting the personal comfort and even safety of the members of an astronomical party on their way to a high-level station. In my opinion, the whole of the ascent should not be attempted on one day; the party ought to devote something like a week to inuring themselves

to an elevation of 7,000 to 10,000 feet before proceeding to the more trying height of 14,000 or 15,000 feet. In my own case, very much against my will at the time, I was detained at Arequipa (7,750) feet for a whole week, with the advantageous result that I experienced hardly any inconvenience when from there I went on to Vincocaya, at a height of 14,360 feet. I mention this in particular, as such very unfavorable reports are current regarding the railway journey on the Oroya line from Lima and the mortality among the workmen employed in constructing that very remarkable railway.

As regards the desirable instrumental equipment, there is one point which I desire to emphasize. The mirrors of the reflecting instruments should be made of speculum metal and by no means of silvered glass. Polished silver is incomparably more liable to tarnish than good speculum metal. Besides, even what would be called a good film of silver on glass is in a considerable degree transparent to ultra-violet rays, as was pointed out by Stokes and Cornu many years ago (*Annales de l'École Normale Supérieure*, ser. II, tome IX, 1880, pp. 22–23). Respecting the behavior of such a film with regard to the infra-red rays I have no knowledge, but doubtless your own Professor Langley has had abundant experience on this point. On the other hand, mirrors of speculum metal reflect the low grade heat rays of the moon, and all other rays up to the extreme known limits of the ultra-violet, with apparent equal completeness. Moreover, a mirror of speculum metal, when made of the proper alloy and well polished, is, under proper care, one of the most permanent of known optical appliances. I have before me the Cassegrain mirror of a reflecting telescope of 6 inches aperture, made by Short in 1745; both it and the other mirrors of that telescope are, to use the words of Dr. Dreyer, "as bright as if they had been polished yesterday." I must add, however, that the telescope, which formerly belonged to King George III, and is now at the Armagh Observatory, seems to have been but rarely used. We have here, however, a Gregorian reflector made by Cary something over 100 years ago, which is frequently used by us for watching the timeball, and though never repolished, is still so bright that one would hesitate to relegate it to the polisher. We have also the magnificent 5½-inch grating presented by the late Professor Rowland more than 20 years ago, also made of speculum metal, which is practically as good as when it was first received by Lord Crawford, though it has been in very frequent use. During one course of observations this grating was exposed to the fumes from peaty ground, which we

found very injurious to silver on glass, but which did not perceptibly affect the brilliancy of the surface of the grating. Doubtless you will be able to learn the exact composition of the alloy used for the Rowland gratings; but probably the combination of 4 atoms of copper with 1 atom of tin, recommended by the late Lord Rosse in his account of the construction of the great telescope at Birr Castle, would yield an alloy capable of retaining its polish for many years. From Lord Rosse's own account it seems that he himself used a somewhat softer alloy, with the consequence that the larger mirrors made by him required repolishing about once every two years; but I may mention that the night air at Birr Castle Observatory is usually very damp, and that owing to the great mass of the mirrors they are very liable to get dewed. This brings me to another part of the subject.

A serious objection to metallic specula, as usually constructed, is their great weight; but this difficulty may be largely remedied by giving the metal a more suitable form than that of a simple circular slab or disc. By the use of suitable sand (such as is used in the production of the highest class of bronze castings) there should be no difficulty in casting a speculum with deep ribs on the back, which would be much lighter and relatively stiffer than a disc of the same diameter. For a mirror 54 inches in diameter, I would suggest making the ribs and face of a uniform thickness of ½ inch. By giving the speculum a total depth of 8 inches, it would probably be stiffer than any mirror yet cast, and with a suitable arrangement of the ribs would weigh about 1,200 pounds. Of course, I assume that the "metal" would be cast face downwards on a "bed of hoops" of the proper curvature, as practiced by the late Lord Rosse, to insure that the surface to be ground and polished should be perfectly sound and almost exactly of the desired form. The possibility of casting a speculum of this shape was, in a great measure, set at rest by an experiment made by the present Lord Rosse, who cast an elliptical flat mirror some 11 inches by 8 inches with a ribbed back, some thirty years ago. This mirror was perfectly sound and took a very high polish. It was used as a diagonal mirror for the 6-foot Newtonian reflector. If I were trying the experiment I should be inclined to honeycomb the mirror after this fashion, adding a "web" round the outside, but taking care to make every part, including the face of the mirror, of the same thickness to facilitate uniform contraction in cooling. It is almost needless to say that a casting of this kind would need to be carefully annealed. Provided the

pattern were made in two parts, back and front, of cast iron and carefully finished, it is quite possible that a uniform thickness of ⅜ inch would be quite sufficient; this would reduce the weight of the finished speculum to 900 pounds. Probably the chief practical difficulty in making such a casting will be so to arrange the mold that it will readily yield to the contracting speculum metal. Very much will depend on the nature and condition of the sand or loam used in forming the mold, but doubtless valuable advice on the whole process could be obtained from an experienced molder who has been accustomed to the production of complicated and fragile castings.

In conclusion, if we regard the rapid progress in spectroscopy of late years associated with the improvement of the diffraction grating, it seems that this is probably no less due to the happy choice of speculum metal for the material of gratings than to the improved accuracy of the ruling. It is therefore reasonable that further advances in many other branches of astrophysics may be expected from a return to the use of the solid metallic reflector in place of the mirrors of silvered glass now so much in favor.

Acknowledgments.

The Committee desires to acknowledge the important advice and suggestions received from Dr. Elihu Thomson, Professor Joseph N. Le Conte, Major George W. Stewart, Professor E. C. Pickering, Professor H. Rubens, Professor F. Paschen, Dr. S. W. Stratton, Mr. T. P. Lukens, Mr. James Gamble Rogers, Mr. C. A. Phillips, Mr. Wm. R. Staats, Miss A. M. Clerke, Professor H. H. Turner, Mr. John Broder, Mr. James Lyman, Dr. G. K. Gilbert, Dr. C. Hart Merriam, and others, particularly from Mr. Charles G. Abbot, Assistant in Charge of the Smithsonian Astrophysical Observatory, who, with the approval of Secretary Langley, furnished a very large amount of detailed information.

PAPERS RELATING TO GEOPHYSICS

REPORT ON GEOPHYSICS

By C. R. Van Hise,

ADVISER IN GEOPHYSICS.

CONTENTS.

In the report of the Advisory Committee for Geophysics submitted last year, the establishment of a geophysical laboratory is somewhat fully considered. This report I was asked to supplement. Before taking up additional points it may be well to briefly summarize the reasons already advanced for the establishment of such a laboratory.

Why a Geophysical Laboratory Should be Established.

In recent years there has been no more striking development than cooperation in industrial enterprises. Whatever may be thought about certain aspects of such cooperation, there is no question that from the point of view of abundant cheaply manufactured products, industrial cooperation has been of enormous advantage.

Science has reached a stage in its development in which cooperation is as essential as cooperation in business. When the sciences were young—indeed, until very recently—work was done in each in comparative independence of others. But the independent advance of the sciences has left unoccupied great intermediate fields. This

is illustrated by the rise, within the last quarter of a century, of physical chemistry and astrophysics. Van't Hoff, Ostwald, and others, seeing that there was a great unoccupied field between physics and chemistry, began the occupation of it. The great results reached by these men placed their names very high in the roll of those who have contributed fundamental ideas to science. More recently we have seen the marvelous rise of astrophysics. The scientific fruits yielded by occupying the ground between astronomy and physics have not been less important than those which have come from occupying the ground between chemistry and physics.

The purpose of a geophysical laboratory is to take possession of the vacant ground between geology and physics and geology and chemistry. So long as geology remained a descriptive science it had little need of chemistry and physics; but the time has now come when geologists are not satisfied with mere descriptions. They desire to interpret the phenomena they see in reference to their causes—in other words, under the principles of physics and chemistry. If this be done the intermediate ground between geology and physics and chemistry must be occupied. This involves cooperation between physicists, chemists, and geologists. If such cooperation be undertaken in a systematic way upon an adequate scale, it is believed that there will be a greater revolution in the science of geology than it has hitherto undergone. Instead of being a descriptive science, it will be a science reduced to order under the principles of physics and chemistry, or, more simply, under the laws of energy. It is also believed that incidentally the sciences of physics and chemistry will be enormously advanced by the investigations undertaken.

As showing the advantages which may come from the cooperation of geologists with scientists in the other branches, there may be mentioned one kind of cooperation, which has already begun upon a considerable scale, which does not necessarily require a laboratory: cooperation between geologists and mathematicians. In the past, many mathematicians have taken up geological problems, but usually their discussions have been unsatisfactory because important geological data were omitted from their premises. But by cooperation with a geologist the mathematician is enabled satisfactorily to apply his mathematics to geological problems because he has a full statement from a competent geologist as to the geological factors which enter into the problems. The mathematician publishes his results and gets full credit for his work. The geologist then applies these results to his geological problems. Thus by action and reaction

between the geologist and the mathematician rapid advance is being made in knowledge of the early history of the earth, knowledge which could not possibly have been reached by geologists alone or by mathematicians alone.

In a geophysical laboratory the geologists would cooperate with chemists and physicists in a similar manner. Expert chemists and physicists would apply chemical and physical methods to the problems of geology. The phenomena and the various conditions under which they were probably produced would so far as possible be made clear to the chemists and physicists in advance of their work. The results reached by the chemists and physicists would then enable the geologists to advance their part of the work. This would lead to further suggestions to the chemists and physicists. One man would not be working for another. The men in the different sciences would be working together for the advancement of knowledge. They would publish their results separately or jointly, as seemed best.

Thus by cooperation, action and reaction between men in the three different departments, the great field between geology and chemistry and physics would be occupied. The fundamental work for the new science of geophysics would be done.

The results which would follow from geophysical work on a large scale are believed to be at least as great as those which have come from occupying the middle ground between physics and chemistry and between astronomy and physics. If a geophysical laboratory were established at Washington, this work would be done in America.

Scope of a Geophysical Laboratory.

The Advisory Committee for Geophysics in its report of a year ago proposed a twofold plan: First, the establishment of a geophysical laboratory at Washington; and, second, cooperation with all existing institutions or men now engaged in geophysical work. These two phases of the subject will be considered in order.

Establishment of a Geophysical Laboratory at Washington.

The report of the Advisory Committee referred to discusses broadly the various problems which should be taken up in a geophysical laboratory and gives a provisional plan for the construction of such a laboratory. The time available to the committee for the

preparation of its report was short, and there were included in the report all important problems of geophysics which occurred to the committee. There was not sufficient time for consultation with geologists as to which of the problems proposed are most pressing, nor with physicists as to which of the problems experience has shown can be attacked with the certainty of securing results. My supplementary work has therefore taken these directions.

The proper construction of a geophysical laboratory is considered by Dr. G. F. Becker, who submits an independent report upon this subject.

Opinions of Geologists as to Importance of a Geophysical Laboratory.

In consulting with geologists as to the lines of work which seem to them essential for the progress of the science of geology, it has been necessary to lay before them the general project of a geophysical laboratory at Washington, so that incidentally their views have been learned as to the importance of the establishment of such a laborar tory to the progress of the sciences of geology, physics, and chemistry. Upon this matter there has been but one opinion: that the establishment of a geophysical laboratory along the lines proposed a year since by your Advisory Committee for Geophysics would be of the very greatest service to science. This view has been expressed by directors of national surveys, presidents of geological societies, presidents of national academies, and many eminent geologists. It seems unnecessary to extend this report by inserting all the statements upon this subject made by various men, but a few may be inserted by way of illustration.

Dr. J. J. H. Teall, Director General of the Geological Survey of Great Britain, says he has "no doubt that a central laboratory of geophysics in Washington, organized in the manner which is suggested in the report of the committee, would contribute very largely to the progress of science." Sir Archibald Geikie, formerly directo- of the same Survey, says in reference to the plan of the committee: "The scheme seems to me well considered and likely to lead to the most important results in the future. * * * International co-operation is destined in the future to play a large part in the progress of science, and the geophysical laboratory at Washington might be made a powerful medium for establishing and fostering this broad spirit of brotherhood in research." Professor Törnebohm, of the

Swedish Geological Survey, says: "In my opinion, the plan of establishing a geophysical laboratory is a grand one. Ably conducted, such an institution may no doubt proffer elucidation on many an obscure question and powerfully promote the progress of many branches of geology and petrology in general." Professor Sederholm, of the Geological Survey of Finland, says: "The enterprise which you hope to start aims at nothing less than to lay a new and in many respects more certain base for geological science. There can be no question about the exceedingly great advantage to the science of such experimental studies. If they have till now played an inconsiderable part in geology, it has been mostly because it has not been possible to make them on a scale in any measure adequate to that of nature." Professor Suess, President of the Royal Academy of Science in Vienna, says: "I would heartily envy the country which might first boast of such an institution."

These opinions are in accord with those expressed by leading physicists and chemists to the Secretary of the Carnegie Institution and published as an appendix to the report of the Advisory Committee for Geophysics. These men, all of whom speak of the importance of geophysics to geology, or to geology and science in general, include Poncairé, Lord Kelvin, Ernst Mach, Becke, Kohlrausch, Van't Hoff, G. H. Darwin, and Nernst.

The establishment of a geophysical laboratory was also discussed with many geologists at the International Congress of Geologists at Vienna this year, and there was but one opinion among representative geologists—that the foundation of a geophysical laboratory would do work of fundamental importance for the science of geology.

Indeed, the Council of the International Congress of Geologists unanimously adopted a statement concerning the subject which was accepted without dissent from any source by the entire Congress. This statement is as follows:

"It is a well known fact that many of the fundamental problems of geology—for example, those concerning uplift and subsidence, mountain making, vulcanology, the deformation and metamorphism of ore deposits—cannot be discussed satisfactorily because of the insufficiency of chemical and physical investigations directed to their solution. Thus, the theory of large strains, either in wholly elastic or in plastic bodies, has never been elucidated, while both chemistry and physics at temperatures above a red heat are almost virgin fields.

"Not only geology, but pure physics, chemistry, and astronomy would greatly benefit by successful researches in these directions. Such researches, however, are of extreme difficulty. They would

require great and long sustained expenditure as well as the organized cooperation of a corps of investigators. No existing university seems to be in a position to prosecute such researches on an adequate scale.

"It is therefore, in the judgment of the Council of the Congrès Géologique International, a matter of the utmost importance to the entire scientific world that some institution should found a well equipped geophysical laboratory for the study of problems of geology involving further researches in chemistry and physics."

In view of the foregoing facts, I think I may unhesitatingly assert that not only the geologists of this country, but the geologists of the world, and all the chemists and physicists who have given any attention to the subject, believe that the results which would be obtained by the establishment of a geophysical laboratory would lead to fundamental advances in the science of geology and great advances in the sciences of physics and chemistry.

The Work of a Geophysical Laboratory.

The general lines of work of a geophysical laboratory are fully set forth in the first report of the Advisory Committee. It has been my aim to supplement this part of the report by ascertaining the nature of the problems which geologists regard as most pressing and which chemists and physicists regard as capable of being successfully attacked. I am not able to make an exhaustive statement in these respects; but, as a result of many conferences, I can specify certain lines along which enough work has been done to make it certain that important results will follow from adequate investigations. While the problems here mentioned are by no means exhaustive, they are sufficiently numerous to show that there is ample work which should be taken up at once to occupy a geophysical laboratory for many years. Some of these problems are as follows:

(1) *The Relations of Liquid and Solid Rocks.*—A line of work along which many geologists are asking for information is that concerning the relations of liquid and solid rocks. They want to know the melting points of rocks, the temperatures at which rocks crystallize from magma, the relative specific gravities of melted and crystallized rocks, the effects of slow cooling upon the crystallization of rocks with and without pressure, the solution of one kind of rock in another, and, in short, all the phenomena which concern the transformation of magma to crystallized rock and of crystallized rock to magma. Upon these various points almost no information is avail-

able, and yet reliable knowledge in reference to them is necessary before the phenomena of vulcanism can be put upon a scientific basis. Experiments in laboratories on a small scale show that this work can be done. But the work has never been done upon an adequate scale, nor is there any probability that it will be done upon an adequate scale, so that the results can be applied to the history of the earth, until a well equipped geophysical laboratory is constructed with sufficient funds to operate on a large scale.

Lord Kelvin suggests that in experimental work involving many of these points at least a cubic foot of the melted rock should be taken. Among other Europeans who mention experimental work along these lines as essential are Dr. Ernest Schwarz, of the Geological Commission of the Cape of Good Hope; Professor Loewinson-Lessing, of the Polytechnic Institute of St. Petersburg; Professor Vogt, of Kristiania, and Professor Suess, of Vienna. Also the necessity for this kind of work has been especially emphasized in America by an important group of geologists, including Adams, Cross, Iddings, Kemp, Lane, Pirsson, Washington, and Wolff. Their views upon this and other pressing investigations in geophysics are set forth in a paper accompanying this report. The carefully systematized, comprehensive plan of work outlined in this paper will be of great assistance to the experimenter if a laboratory is constructed.

Sufficient work has been done by Morosiewitsch, Doelter, Brun, and others to show that an investigation of the relations of fluid and crystallized rocks will be very fruitful. In America, Professor Carl Barus, under the direction of Clarence King, once began investigations upon fluid rock, but this work was unfortunately discontinued because of lack of funds. Little work along this line is being done anywhere simply because of the lack of properly equipped laboratories with adequate funds to carry on such necessarily expensive work. If such work be provided for in a geophysical laboratory at the Carnegie Institution, no one can doubt that scientific results of the first order will be obtained.

(2) *Minerals and Rocks from Aqueous Solutions.*—Another class of investigations is the artificial production of minerals and rocks from aqueous solutions. This involves a study of natural solutions, both those of the sea and those in openings in rocks, in order to determine the conditions under which minerals crystallize from such solutions. Already the study of natural solutions with reference to the crystallization of salt and gypsum has been undertaken by Van't Hoff.

This great chemist has reached many important results, but he points out that very much remains to be done, and especially recommends this line of study to be taken up on an adequate scale in a geophysical laboratory. Experiments should be carried on with aqueous solutions under various pressures and at various temperatures. The higher temperatures should approach those of magmas, in order that the relations of crystallization from magmas and crystallization from water may be learned. It is held by some that there is gradation between these. Sufficient has been done by various workers to show that very important results can be reached by the investigations proposed, and a well organized, comprehensive series of experiments is now needed. It is certain that the conditions under which many of the minerals produced in nature from water solutions can be produced in the laboratory. Only when this is done shall we have an adequate basis upon which to judge of the kinds of minerals that are produced in nature from aqueous solutions and their manner of formation.

The study of natural underground solutions and the artificial production of minerals have a most intimate relation to ore deposits. Already studies along these lines have led to large advances in knowledge of the development of ores. This the men engaged in mining have recognized. Very recent contributions upon this subject have been of great practical importance in the exploration and exploitation of ores. There is unanimity of opinion among geologists that experimental studies on underground solutions and the artificial reproduction of the natural minerals will lead to correct theories of ore deposition and also give results of practical value, the magnitude of which cannot now be estimated.

(3) *The Deformation of Rocks.*—Elaborate experimental work should be done upon the deformation of rocks under different conditions of speed, temperature, pressure, and moisture. At the present time Dr. Frank D. Adams, at Montreal, is engaged in the slow deformation of one rock—marble—on a small scale. Indeed, in this work he has the support of the Carnegie Institution; but experiments along this line need to be carried through long periods of time for many kinds of rock on a much larger scale than heretofore, in order that the results may be applied with safety to the observed deformation of the vast masses of material of the earth. But already sufficient preliminary work has been done to show that this is a field for laboratory investigations which will certainly yield important results to the science of geology.

Dr. James Dewar, Professor of Chemistry in the Royal Institution of Great Britain, is now engaged in testing the strength of rocks at the temperature of liquid air. Already he has reached remarkable (unpublished) results; but he states that the apparatus and equipment at his command are entirely inadequate to carry on experimental work on the deformation of rocks at low temperatures on a scale that such work demands in order to give satisfactory results. He says that if a laboratory of geophysics were established the determination of the breaking strength of various rock masses, by compressive, tensile, and tortional stresses, should be made at low temperatures. He says further that a complete determination of the elastic constants of rocks at different temperatures, under stresses of various kinds, should be made. Professor Dewar states that by the low temperature work upon very small masses of a few varieties of rock in his laboratory he expects to show merely that very important results can be reached by this line of work, and thus to lay out a great field for extensive work along the same line. Such work as that proposed by Professor Dewar is not provided for anywhere in the world. Such work is especially appropriate to a geophysical laboratory.

(4) *The Constants of Rocks.*—Another set of problems which many geologists desire attacked concern the constants of rocks at various temperatures and pressures, such as their densities, their coefficients of expansion, their specific heats, conductivities, etc. The lack of knowledge of these constants, which can certainly be determined by experiment, has stood in the way of the progress of geology in various directions. The need for work along these lines is especially emphasized by Lord Kelvin and Professor Dewar, and is discussed by Dr. Becker in his report of last year.

Basis of Selection of Problems Suggested.

In mentioning the foregoing broad lines of investigation I have confined my statements to those which are urgently demanded by many geologists as necessary for the progress of the science. They represent the consensus of opinion of the many geologists with whom I have conferred rather than my own views. I have purposely omitted the problems mentioned in the first report of the Advisory Committee for Geophysics that are somewhat more remote from the present pressing problems of the geologist and the student of ore deposits.

In order to recall some of the lines of work which are not here

considered, it may be said that all of the great problems concerning the atmosphere set forth in the first report of the committee are wholly omitted; also the great problems dealing with the interior of the earth have been ignored. Finally, all the problems along the border line of astronomy and geology which concern the early history of the earth have been omitted. By these omissions I do not mean to imply that each of the lines is not of profound importance. Indeed, I believe that all should ultimately be taken up in a geophysical laboratory. It may be taken for granted that a deeper insight into the order of the universe is a sufficient reason—indeed, is the most important and fundamental reason—for investigations in science. All of these omitted lines fall within this class of studies, but the report of the Advisory Committee for Geophysics has already fully covered these problems. A special purpose of this supplementary report is to emphasize the point that there are many problems of immediate importance to the science of geology and to a knowledge of ore deposits which deal with the part of the earth that we can see, concerning which experimental work is demanded by the geologists of the world, because lack of such work stands in the way of the advance of science.

Cooperation in Geophysical Work.

I shall next consider the second part of the proposal of the Advisory Committee of last year—that of cooperation in geophysics. The plan of the Advisory Committee provided for the use of branch laboratories in various parts of the world. It was thought it might be necessary to construct an occasional small branch laboratory, but, so far as possible, it was proposed that all existing laboratories should be utilized to the fullest extent; also the plan of cooperation provided that the central laboratory at Washington should be a clearing-house for the geophysical work of the world. This clearing house would acquire accurate information as to the geophysical work being done in all laboratories of every country. Any scientist who wished to know the present status of knowledge in reference to any problem and what others are doing, so that he might take up work which should not duplicate that already done or being done by others, could apply to the Carnegie Institution at Washington and obtain the needed information.

No part of the general plan of the Advisory Committee has received more universal approval by geologists, physicists, and

chemists of various countries than its proposal for cooperation in geophysics.

Branch Laboratories.

It has been suggested, especially by Professor Loewinson-Lessing, that a branch laboratory in the Hawaiian Islands, which are now a possession of the United States, would give unexampled opportunities for the study of vulcanism. The majority of the present living volcanoes are comparatively small. In Hawaii are the greatest of the existing volcanoes—those that are most nearly comparable to the ones which must have existed when the vast lava plateaus of various parts of the world were produced. If a branch laboratory were established in Hawaii, there can be no question that the knowledge of the phenomena and causes of vulcanism would be greatly advanced.

Seismology.

Another line along which cooperation is especially urged by various European geologists is seismology. It is unnecessary to urge the importance of seismological investigations both to science and to constructional work. At the present time there are many seismological stations scattered over various parts of the world. However, for an adequate study of earth tremors it is advisable that additional stations should be established at a number of wisely selected places in the more remote parts of the earth. Professor Milne at Shide, Isle of Wight, has for many years been receiving records of a large number of the instruments now in use, but the work has now become too large for him to carry, and he asks for assistance. Recently it was arranged that Strassburg be a center of information for seismology, but some countries have refused to cooperate in this plan. The time is now ripe for some institution with adequate funds to arrange a broad scheme of cooperation between the various interests and to be the medium which harmonizes them, and thus to systematize the seismological work of the world. Many have said to me that the unique position of the Carnegie Institution, free from all entanglements and prejudices, places this institution in by far the most advantageous situation to accomplish this work. Indeed, a number of geologists have said that, so far as they can see, unless the Carnegie Institution takes up this work, the same chaotic condition of affairs that has existed in the past will continue. It is believed that in securing the cooperation of all the men engaged in

seismology and in coordinating all of the work on seismology, a laboratory of geophysics at Washington would find one of its greatest opportunities.

Cost.

As to the cost of a geophysical laboratory, the committee of last year submitted an estimate which it thought sufficient to provide adequately for the great plans laid out by it. If the scope of the proposed laboratory be confined to the more pressing lines of work indicated in this supplementary report and to other problems of an equally pressing character, and the more remote problems are ignored for the present, the cost of a laboratory can be very considerably reduced. Indeed, to get very important results, it is not necessary that all of the problems discussed in this report shall be taken up at once. If a laboratory were established, the governing body could best decide which problems should be undertaken after it was known how much money was available. After deliberate consideration of the matter from the minimum point of view, rather than from the point of view of what is desirable, I have come to the conclusion that work of very great importance can be done in geophysics for $50,000 per annum; but in order not to greatly delay work, it is strongly urged that $100,000 be appropriated toward a building. This would make it possible to begin work on a productive scale much sooner than if only $50,000 per annum were appropriated, and from this fund it were necessary to construct the building and purchase apparatus.

My recommendation is, therefore, that there be appropriated for the construction and maintenance of a geophysical laboratory $100,000 and $50,000, with the expectation that the latter appropriation will be an annual one.

While with the amount suggested it will not be possible to press the various lines of geophysical work with the speed which many strongly hold to be exceedingly desirable, I feel confident that great, indeed revolutionary, results to the science of geology will be obtained.

CONSTRUCTION OF GEOPHYSICAL LABORATORY

REPORT BY GEORGE F. BECKER.

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ORIGIN OF REPORT.

In March, 1903, Mr. Walcott requested me to cooperate with Professor Van Hise in gathering information abroad with reference to the construction suitable for a geophysical laboratory, and in regard to the problems which could be profitably studied in such a laboratory were it to be built. After consultation with Professor Van Hise it was decided that the most important features of laboratories are the means adopted to secure stability of piers and the methods of obtaining constant temperatures. Laboratory construction must determine in what measure stability of instruments and constancy of temperature can be attained. On the other hand, to the investigators who occupy the laboratory after completion must be left in large measure the details of apparatus and of methods of research.

Laboratory construction is a matter of extreme importance and one which has been, relatively speaking, neglected. Vast ingenuity has been applied to the perfection of apparatus, while little pains has been taken to provide for that freedom from mechanical and thermal disturbance without which many instruments of precision cannot possibly give the best results of which they are capable. Hence also the work done in an ill constructed laboratory, other things being equal, will be inferior in quantity and quality as compared with that achieved in a suitable building.

Institutions Visited.

In accordance with the plans thus laid, I visited the Cavendish laboratory at Cambridge, the laboratory of the Sorbonne and the astronomical observatory at Paris, the laboratory of the Bureau International des Poids et Mésures at Sèvres, the physical laboratories of the universities of Strasburg and Würzburg, the geophysical laboratory of Göttingen, the laboratories of the Physikalisch-Technische Reichsanstalt of Charlottenburg, the laboratory of the Normal Aichungs Komission of the same place, the Astrophysical observatory of Potsdam, the physical laboratory of the University of Leipzig, the cryogenic laboratory, and the Astronomical observatory of Leiden.

So far as possible, I consulted the chief physicists or astronomers of these institutions on the main points of my inquiry. Professor J. J. Thomson was absent and Professor Onnes, of the cryogenic laboratory, was engaged on the day of my visit. On the other hand, I had instructive interviews with Professor George H. Darwin, Professor Charles Chree, Sir Archibald Geikie, Professor E. Suess, and others.

Nearly everywhere I have found physicists dissatisfied with the construction of their laboratories and fully persuaded that radical improvements are possible. Satisfactorily firm piers have been constructed only at Potsdam and Leiden, and in both these places the successful result seems due rather to natural conditions than to peculiarities of construction. Fairly uniform temperatures, excepting in underground chambers, have not been attained, although a majority of physicists are of opinion that they might be brought about. Heating and ventilation are usually no better than in any ordinary office building.

An ideal laboratory would be free from magnetic, electrical, or mechanical disturbances and from unintentional changes of temper-

ature. It would be possible to maintain any room at any desired temperature for any desired period of time consistent with good lighting and ventilation. Such conditions cannot be fully realized.

MAGNETIC DISTURBANCES.

Magnetic observations are so subject to disturbances that in practice it is found needful to provide for them in separate buildings, free from iron and as remote as possible from industrial establishments. For more general laboratories, therefore, purely magnetic disturbances may be left out of account, and iron may be freely employed in construction so far as it does not lend itself to the propagation of mechanical vibrations.

ELECTRICAL DISTURBANCES.

Electrical disturbances are of two orders of magnitude: Trolley lines using an earth current produce serious electrical disturbances at a distance of at least one mile, while trolley lines with a double overhead or underground metallic circuit, as well as carriages deriving power from storage batteries, are innocuous at a distance of only a few hundred feet.

TWO DESIRABLE CONSTANT TEMPERATURES.

Except in deep subterranean chambers, it is difficult at best to maintain uniform temperatures. Far greater is the difficulty of changing the temperature of a room at will, for a very long time must elapse before the massive masonry of the walls and piers acquires the new temperature. For these reasons it appears inexpedient to attempt more than two temperatures in any laboratory, except perhaps in one small room. One of these temperatures is the mean temperature of the subsoil, say 9° or 10° C. in temperate latitudes, and the other is a comfortable temperature for work, say 20° C.

ANNUAL MEAN TEMPERATURE.

The maintenance of the lower temperature with extremely slow variations of a few degrees is not difficult in cellar or subcellar spaces. It is also possible, as Professor Wiechert has shown, to keep the air in such spaces moderately dry. Mr. Wiechert admits the air to his seismometer house through a galvanized-iron tube,

which is convoluted between the double walls of the house and provided with drips. The tube enters the inner chamber near the ceiling and passes round the entire inner space, always at a slight slope, so that all condensed moisture trickles backward. From the air tube air escapes into the instrument room through small holes in the side of the tube. The result is, for some purposes, a very satisfactory one, attained almost without expense. In a larger laboratory a somewhat different method would probably be more convenient and more effective.

Difficulties of Varying Temperatures.

Attempts have been made in Europe to control the temperature of large apartments by providing them with double metallic walls in which hot or cold solutions circulate, but these efforts have not been successful. In warm weather, when cold solutions must be employed, the walls drip with moisture, the instruments suffer, and the operators fall ill. This would be avoided by supplying the apartment with air not merely cooled but dried, just as a room in winter may be heated by a hot-air furnace.

In this country more attention has been given than in Europe to cooling apartments with dry air. The Bureau of Standards has developed arrangements for this purpose which will be in operation in a few weeks, and it is said that the Stock Exchange in New York is being similarly equipped. The experience obtained by the Bureau of Standards should be carefully considered before any specific plan is adopted for a geophysical laboratory.

Importance of Uniform Temperatures.

In my opinion, a modern laboratory should be supplied in summer with dry, cool air, the temperature of which is under control. Such air of appropriate temperature should be admitted to the cold subterranean chambers when required, and should be furnished to the ordinary laboratories in such quantity as to keep their temperature down to 20° C. = 68° F. In winter the rooms must, as a matter of course, be warmed. If the problem of maintaining a laboratory at constant temperature is not wholly simple, it is surely of small complexity as compared with those of physical research, and it cannot be doubted that were its solution requisite to the success of a commercial enterprise, an appropriate method would soon be developed. Yet it is unquestionable that physical research would

proceed much more rapidly and effectively in a laboratory of fairly constant temperature. Some physicists, indeed, maintain that it is sufficient to attempt constant temperatures only within pieces of apparatus, but in this view I cannot agree. A standard bar, for example, may be measured in a case kept nearly at constant temperature by circulating liquids or by electric resistance; but this temperature depends in part on the radiation of the case, and this on the temperature of the apartment. Again, the accuracy of galvanometers, and all similar apparatus, is greatly promoted by a substantially uniform temperature.

Special Difficulties in America.

In the eastern United States the natural atmospheric temperature varies so enormously and so rapidly as greatly to interfere both with the accuracy of instruments and the capacity of observers. Heat flows from piers to instruments, or in the reverse direction, so fast as to be most disturbing and wholly incalculable, and this flux renders the more minute measurements most uncertain. Thus, even more than in Europe, it is desirable that an American laboratory of the highest class should be isothermic.

Avoidance of Heat Flux.

The maintenance of a temperature of 20° C. in a laboratory is attended with other difficulties besides that of supplying cool or warm air. In the laboratory of the Normal Aichungs Kommission it has been found that the flow of heat downward through the piers is a very disturbing factor at the best, and I there received the excellent suggestion that the exposed portion of the piers should be made of metal, in order that it should readily take on the temperature of the observing room. The metal plate should stand on three points, and the stratum underlying them should be of the most non-conductive material which can be found. Hard magnesia brick is almost ideal in this respect, and I suggest that the masonry of the piers be faced with this material.

Notes on Ventilation.

Insulation, excepting in very deep subcellars, is not sufficient to establish uniformity of temperature, even in Europe. In the eastern United States, the annual variation being much greater than on the other side of the Atlantic, insulation is still less effective.

Modern American engineers have reached the conclusion that either in warming or cooling apartments diffusion of the air, unaided by stirring, cannot be relied upon to produce substantial uniformity of temperature. Air of nearly the desired temperature must be forced to circulate through the room at velocities which are sensible, but are not necessarily great enough to constitute deleterious drafts. In my opinion, cold air should be admitted in summer at numerous openings near the ceiling of rooms, while heating should be effected in winter by warm air entering at many openings in or near the floor. Ventilation should not be left to natural draft alone, for this sometimes fails; it should always be possible to control it by electric fans or other engines. Thermostats should be employed, but they should be of very solid and durable construction, and they should be carefully checked until found entirely satisfactory.

Suggestion that Vibration of Piers be Damped.

The subject of stable piers is one of the most vital importance to laboratory construction, yet it has been most imperfectly investigated. There is no question whatever that superficial vibrations of the soil are largely cut off by a trench excavated about a pier, and for this reason the whole new laboratory at Leipzig is inclosed in a trench. On the other hand, the base of a pier under ordinary conditions is its stablest portion, so that the top of a tall pier vibrates far more than its base. It occurred to me that the vibrations of a tall pier might be damped, for example, by filling the trench about it with coarsely ground cork or some similar substance. I seemed to see an illustration of this principle in Wiechert's seismometer, an instrument which is, of course, intended to respond to the most minute vibrations. Except for a very essential device, this seismometer, when once agitated, would continue to vibrate for a long time with a period of its own. This would, of course, defeat its purpose. To render it a "dead-beat" instrument, it is damped by air cataracts, and thus records only the tremors communicated to it by the earth. Now, why should a pier not also be damped by air cataracts, cork-packing, or other means? In Leiden I met what seems a most surprising confirmation of this idea. The Cryogenic Laboratory and the astronomical observatory are built on soil which overlies some 40 feet of soft mud resting on sand and clay. This seems a most unpromising position for stability, yet experience has shown that it is not so. The piers are built on groups of piles driven well down into the

sand and sheathed in planks. The buildings are built on systems of piles inclosing those of the piers. Now these piers are so stable that in the Cryogenic Laboratory observations with the most delicate galvanometers are entirely practicable when five condensing pumps are at work in a room only a few yards distant, while in the observatory there is not the least trouble in using the quicksilver mirror.

On the other hand, at the Bureau des Poids et Mésures wagons on a high road some hundreds of yards away shake the piers, and the quicksilver mirror is seriously disturbed even at the bottom of the catacombs near the Paris Observatory. It would seem to me that the mud underlying Leiden damps the vibrations of the piers much as cataracts would do, and that this is the only probable explanation of their success, which I understand to have been attained without special reference to the efficacy of mud for this purpose.

Research Called for.

The subject is one needing and deserving research. In the well-known Julius suspension, means are adopted both to secure damping and to place the instrument in a node of vibration. The extraordinary efficacy of this suspension is well known. It appears to me perfectly possible to devise piers, after proper investigation, in which vibrations will likewise be damped and in which the upper surface will lie in a node.

While the results in Leiden show that mud underlain by firmer material is not a bad foundation for a laboratory, no one, I take it, would deliberately select such a situation if solid rock or a firm saprolite (decomposed rock in place) were available.

Construction of Laboratory Building.

All the most delicate experiments of a laboratory would be carried on on piers and in the basement or the first floor of the building, but great stability sufficient for a large class of experiments can be obtained in a second floor by the use of masonry arches. For this reason I cannot recommend for a laboratory the steel beam construction now used in ordinary buildings. In such buildings the oscillations due to wind would be very sensible, and any jars due to moving apparatus or similar causes would be communicated to other portions of the building much more readily than in an arched construction. Professor Wiechert has measured quantitatively the deflection of his main laboratory building by the wind.

I find physicists most positively of the opinion that the walls of laboratories should be broken by as few openings as possible. No flues for ventilation or any other purpose should be included in the thickness of the walls. Plumbing and piping should be placed in wells reserved for the purpose and conveniently accessible.

As material for the construction of laboratories, nothing seems preferable to good brick well laid. Experiments long ago made by officers of the Coast and Geodetic Survey show that sandstone should be rigidly excluded from every portion of the building. I know of no objection to concrete ; provided, however, that the stone used be sharply angular and on no account consist of rounded pebbles.

The walls of a laboratory should be very thick and massive, not less than three feet in the lower story. All doors and windows should be double. It is the habit to inclose the constant-temperature rooms in double walls, but I believe that a single wall surrounded by cork brick would be equally good. This admirable material is much used as a non-conductor ; for instance, in the insulation of the clocks of the Paris observatory.

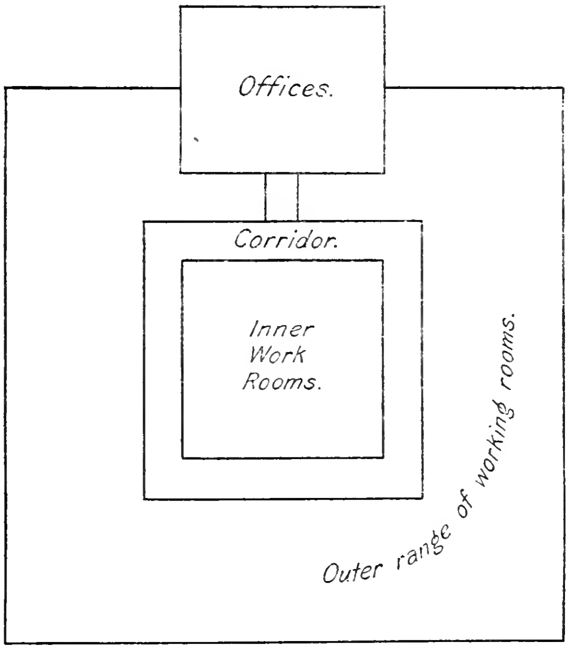
Subdivisibility.—An important feature of laboratory construction is the subdivisibility of the work rooms. As investigations succeed one another, rooms of different dimensions become desirable. The smallest room requires one window and one door, with independent gas, water, and electrical connections. So far as possible, it should be arranged that such rooms may be separated from one another by partitions of hollow brick, which can be removed without interfering with the structure of the building. Thus the size of the work rooms can be reduced to a minimum or enlarged as required at a trifling expense.

Basement Work Rooms.—The basement of a laboratory should be of sufficient height to be used conveniently as working space. In a well drained locality it affords the very best of working rooms.

Interior Work Rooms.—Opinions differ somewhat as to the best general plan for a laboratory. A few physicists object to internal rooms lighted only from above, considering them gloomy and subject to accident from the breaking of skylights. On the other hand, such rooms afford the best constant-temperature spaces and are freest from mechanical disturbance. Again, so-called wire glass and other simple expedients give ample protection from danger of falling glass. For these reasons a majority of physicists approve of internal rooms surrounded by a corridor, and this again inclosed by outer working rooms. The corridor, among other advantages, affords

most desirable space for placing cases in which to store apparatus and supplies.

In my opinion, the offices of a laboratory which are to be frequented by outsiders should be partially isolated from the main building, as indicated in the following rough diagram, so that doors leading into the main building should be opened only when this is unavoidable.



Number of Stories.—Three stories seem necessary and sufficient for a laboratory. The lowest, or basement, story should be on the ground, with one or two deep subterranean chambers beneath for secular experiments. The second story should be on arches above the basement, and would afford excellent working rooms with piers of fair stability. A third story would serve to protect the second and can be usefully employed for photographic work and experiments not dependent upon piers.

Estimates of Expense.—Estimates of the expense of building a laboratory were given in the project submitted by me to the Institution nearly a year since. These were based on the cost of the laboratory of the Reichsanstalt, and were set down at $250,000. I know of no way to improve upon this result except by having an architect draw

preliminary plans for a building. The heavy walls and arches would be expensive, but the absence of flues and the flat wall surfaces called for would tend to keep down the cost.

Problems of Geophysics.

I have consulted Messrs. George H. Darwin, Charles Chree, Kohlrausch, and others on the problems which can be attacked in a geophysical laboratory with fair prospects of success. They appear to agree with the views set forth in Appendix 1 to Report of Advisory Committee on Geophysics (Carnegie Institution Year Book, No. 1, pp. 44–58). In particular, Professor Chree, who is the leading expert on the theory of the vibrations of a sphere, agrees with me that some years of mathematical work must be done before seismological observations can be duly interpreted. Such mathematical investigation should certainly be undertaken as soon as possible. Investigations into finite stress and strain, rupture, the steady flow of heat, diffusivity, aqueo-igneous and dry fusion, together with the whole chemistry and physics of high temperatures, afford brilliant prospects of important results and wide applications of the conclusions drawn to terrestrial problems.

It would be easy to enumerate many specific problems, and this I have done in a manuscript report to Mr. Walcott, entitled "Remarks on Geophysics," dated March 21, 1902 (not published), but the work indicated in the last paragraph would worthily occupy any institution for many years, and a consideration of what should be undertaken when this is done may well be postponed.

Washington, D. C., *October 2, 1903.*

GEOPHYSICAL INVESTIGATIONS SUGGESTED

In the interests of the Science of Petrology several of us, who have devoted much of our time and energies to petrological matters, call attention to a statement of some of the problems in the line of physico-chemical investigation which could be undertaken in a properly equipped laboratory and urge the importance of laying the matter before the geological committee of the Carnegie Institution in order that it may be favorably inclined toward the investigations proposed, some of which are already being carried on under the patronage of the Carnegie Institution.

It will be noted that many of them are of far-reaching importance to the advancement of general geological problems, of which the knowledge of the properties and history of rocks forms a very considerable part, the problems connected with rocks affecting the whole geological system. It therefore happens that problems stated in this connection may naturally be restated in others.

INVESTIGATION OF IGNEOUS ROCKS.

I. Determination of Changes of Condition Accompanying Changes of Temperature.

A. *With normal atmospheric pressure.*

1. Change of volume and of viscosity; that is, the rigidity, melting point, and liquidity of—
    - (*a*) *Rock glasses* of known chemical composition, corresponding to known igneous rocks.
    - (*b*) *Glasses* of single minerals.
    - (*c*) *Crystals* of single minerals.

This involves fusion in open crucibles with the determination of temperature thermo-electrically by methods employed by Dr. Barus and Dr. Day in the laboratory of the U. S. Geological Survey.

B. *With high pressures in closed vessels.*

The effect of increasing pressure on the volume and the viscosity—rigidity, melting point, liquidity—of *rock glasses*, and *glasses and crystals* of known *minerals*.

II. Determination of the Behavior of Solutions of Minerals in One Another.

A. *With normal atmospheric pressure.*

1. The temperature at which two minerals will go into solution in one another, and the temperature of solidification of the mixed solution.

   For various pairs of rock-making minerals such as—

   Quartz and orthoclase.
   Orthoclase and albite.
   Orthoclase and pyroxene.
   Pyroxene and olivine.

2. The point of saturation in each case for various temperatures.
3. The solution of gases in liquid minerals and rocks.
   - (*a*) The rate of absorption (see also Diffusion, heading III) and the limit of saturation for different temperatures.
   - (*b*) The relation between the composition of the liquid mineral or rock and the limit of saturation.
   - (*c*) The effect of dissolved gases on the viscosity of the liquid rocks.

B. *With high pressure in closed vessels.*

The influence of increasing pressure on the solution of various minerals in one another, on the saturation and solidifying point, and on the solution of gases in the liquid rock and on the limit of saturation, especially the effect of dissolved gases in liquid rock under high pressure on the viscosity of the liquid. (See Diffusion, below.)

III. Determination of the Rates of Diffusion in Liquid Rocks and Minerals.

A. *With normal atmospheric pressure.*

1. The rate of *heat* conductivity in solid and liquid rocks and minerals at various temperatures.
2. The rate of absorption and transmission of various gases in liquid rocks of different compositions.

3. The rate of molecular diffusion in liquid rocks by osmotic pressure. The solution and diffusion of molecules of one kind of mineral in liquid rocks or minerals of other kinds.

   This has a direct bearing on the solution and diffusion of rocks in molten magmas, and on the theory of rock synthesis, and on that of magmatic intrusion by solution of the surrounding rocks.

4. The recognition of possible colloids in liquid rocks, which may form in the presence of gases, such as water gas, under pressure; the colloidal condition of silicon hydroxide, aluminum hydroxide, and ferric hydroxide being easily conceivable. It is also possible that more complex, alumino-silicate molecules may become colloidal. This might be detected electrolitically in molten magmas in the presence of water under pressure.

   The bearing of this on the problem of differential diffusion and differentiation is important on account of its bearing on the question of the origin of different kinds of igneous rocks.

B. *With high pressure in closed vessels.*

1. The effect of increasing pressure on heat conductivity in solid and liquid rocks.
2. The effect of increasing pressure on the diffusion of gases in liquid rocks.
3. The effect of increasing pressure on molecular diffusion by osmotic pressure.
4. The rate of molecular diffusivity at high temperatures and high pressures in liquid rocks containing various amounts of gases.

   This has an important bearing on the probably high rate of differentiation in the more liquid molten magmas.

IV. Crystallization from Liquid Rocks with Temperature Determinations and Observation of the Time Element.

A. *With normal atmospheric pressure.*

1. The determination of the temperature of saturation and the rate of crystallization of *simple minerals* in cooling liquids of the same composition.

Open crucibles.—The production of crystals of quartz, orthoclase, albite, as well as those of augite, olivine, anorthite, etc.

2. The crystallization of several heteromorphous minerals from cooling mixed solutions of the same. Examples:
   Anorthite and olivine.
   Leucite and diopside.
   (*a*) The order of crystallization related to the relative amounts of the two mixed minerals.
   (*b*) The rate of crystallization. } In the same relations.
   (*c*) The habit of the crystals. }
3. The crystallization of isomorphous minerals from mixed solutions. Such as—
   The series of albite—anorthite feldspars.
   The series of pyroxenes.
   The series of amphiboles.
   (*a*) The order of crystallization.
   (*b*) The production of distinct kinds, or of crystals of average composition. That is, the production of zonally different crystals, such as the lime-soda feldspars in many cases, or of a homogeneous crystal of intermediate composition. The relation between these modes of crystallization and the *rate* of crystallization.

B. *With high pressure in closed vessels.*

1. The effect of the presence of dissolved gases on the crystallization of anhydrous crystals from liquid rocks.
2. The effect of dissolved gases on the crystallization of—
   (*a*) Hornblende as opposed to pyroxene.
   (*b*) Biotite as opposed to orthoclase and hypersthene, etc.
3. The relation between pressure and the chemical character of the minerals or salts crystallizing from a mixed solution.
4. The possible crystallization of hydrous minerals such as epidote, analcite, muscovite, from liquid rocks under pressure (to account for "primary" epidote and analcite in igneous rocks).
5. The possible crystallization of calcite and other carbonates from liquid rocks.

6. The relation between the *size of crystals* and—
   (*a*) The *composition* of the mineral liquid—the solvent.
   (*b*) The *rate of cooling*.
   (*c*) The *mobility* of the molten magma, and its content of gas.
   (*d*) The pressure.
7. The relation between the *habit* (shape) of crystals and–
   (*a*) The *composition* of the mother liquor.
   (*b*) The *rate* of growth.
   (*c*) The molecular *mobility* of the mother liquor.
   (*d*) The absence or presence of currents or motion at the time of crystallization.
   (*e*) The pressure.
8. The *texture* of rocks in its relation to—
   (*a*) The *development of phenocrysts* by partial crystallization of the magma at one rate and the solidification of the remainder at another rate (to be effected by change of physical environment, change of temperature, or pressure).
   (*b*) The diverse rates of growth of diverse minerals in mixed solutions.
   (*c*) Synchronous intergrowths of mixed salts, as of quartz and orthoclase.
   (*d*) The effect of localized "crystallizers" in producing centers of crystallization resulting in spherulites, segregations, etc.
   (*e*) The possible development of "protoclastic" texture in moving crystallizing magma.

The physical investigations should be accompanied by close chemical studies. The precise chemical composition of all material experimented on should be determined.

The rôle played by certain elements in minerals should be investigated, such as the possibility of Al_2O_3 behaving as an acid radical in the feldspars, etc.

The molecular constitution of the more complex silicates may be investigated by means of the determination of the specific heat of the minerals.

INVESTIGATION OF METAMORPHIC ROCKS.

I. *Determination of heat conductivity in solid rocks and crystals.*

II. *Porosity of rocks.*

(*a*) With regard to the transmission and circulation of water.

(*b*) With regard to the transmission of gases.

III. *Solubility of rock-making minerals in water.*

(*a*) At various temperatures.

(*b*) At various pressures.

IV. *Solubility of rock-making minerals in vapors.*

(*a*) At various temperatures.

(*b*) At various pressures.

(*c*) Also in mixtures of water and gases of various kinds, dilute acids.

V. *Solution and recrystallization.*

Chemical reactions at various temperatures and pressures.

Examples: *Wollastonite* converted to *calcite*.
Calcite converted to *wollastonite*.

VI. *Physical changes accompanying hydration and dehydration.*

VII. *Growth of large crystals at the expense of small ones under static conditions.*

The development of large, pseudoporphyritic crystals, ottrelite, staurolite, garnet, etc., in certain schists.

VIII. *Solution and recrystallization of strained crystals of rock-making minerals, and of other salts.*

IX. *Changes resulting from differential stresses.*

1. Determination of the relation between the *rate* of deformation and the strength of the deformed rock.
2. Relation between the amount of deformation and the degree of pressure producing it.
3. The resistance offered by rocks to deformation.
4. The deformation of hot rocks in the presence of water.
5. The relative degrees of deformation experienced by given rocks at different temperatures and with different content of water, for different pressures, with limestone, marble, sandstone, granite, etc.

6. The possible production of gneissic texture in granite, and in basic rocks.
7. The investigation of the possible recrystallization of gypsum under stresses with varying conditions of temperature and moisture.
8. The same with respect to ice.
9. The comparison of the original structure and the structure resulting from deformation in the case of rocks with those seen under similar circumstances in the case of metals and alloys.
10. Theories of the effects of mutual compression on rocks of dissimilar character and diverse grades of rigidity as bearing on the dynamics of mountain-making.

X. *Effects of motion* (flowage) *on crystallization.*

1. Relation of the arrangement of primary material in bands, stratification, to the development of foliation, etc.
2. Experiments on the production of secondary foliation in rocks already foliated.

XI. *Determination of the possible relation between chemical affinities and stress.*

XII. *Determination as to the formation of graphite in metamorphic rocks*, whether it requires the presence of organic material under conditions of pressure metamorphism.

There are, of course, numerous other lines of investigation, as well as elaborations of those here suggested, which may be carried on to the substantial advancement of our knowledge of rocks and minerals and of the consequent physical character of the earth as a whole.

FRANK D. ADAMS.
WHITMAN CROSS.
JOSEPH P. IDDINGS.
JAMES F. KEMP.
ALFRED C. LANE.
LOUIS V. PIRSSON.
HENRY S. WASHINGTON.
JOHN E. WOLFF.

OCTOBER 1, 1903.

PROPOSED INTERNATIONAL MAGNETIC BUREAU

By L. A. Bauer.

CONTENTS.

Purpose.

The purpose of the proposed Bureau is to investigate such problems of world wide interest as relate to the magnetic and electric condition of the earth and its atmosphere, not specifically the subject of inquiry of any one country, but of international concern and benefit.

Some such problems are:

(*a*) *A magnetic survey of ocean areas and unexplored regions.*—A problem of immediate importance in view of the extensive magnetic work now being carried on in many countries and the various Arctic and Antarctic expeditions in order to complete the magnetic survey of the globe, and thus render possible the construction of more accurate and comprehensive charts. When the great part played by the magnetic needle in navigation and exploration is considered, and the intimate relationship between terrestrial magnetism and other sciences, such as meteorology, geology, and astronomy, is so called

to mind, it is unnecessary to dwell at greater length upon the practical and theoretical importance of this problem. No magnetic data have been obtained on the ocean areas since the advent of iron ships, except from occasional expeditions. Our present lines of equal magnetic declination over these waters depend almost entirely upon the data acquired in wooden ships, fifty to one hundred years ago.

(*b*) *International observations of the variations.*—An exhaustive study of the numerous variations of the earth's magnetism and electricity can be successfully pursued only by international cooperation under the direction of a central or international bureau, so as to secure uniformity in observation and reduction, and to insure the prompt coordination and publication of results. All past international work, as, for example, that conducted by many nations during the international "polar year" of 1882–'83, has suffered from the lack of a central or head bureau. During that year thousands of observations were made; the full utilization of those has not yet taken place, although they were made twenty years ago. Again, the remarkable activity now manifested over the entire globe is not being guided by any one internationally recognized bureau; we may therefore expect that, after the observations are made, it will be many years before the results of this vast amount of work will be known. Printed international forms for guidance in observation, reduction, and publication would prevent much waste of effort.

Under this heading is also embraced the establishment of secular variation or repeat stations throughout the globe, at which magnetic observations would be repeated at regular intervals, and thus make it possible to obtain the data necessary to keep magnetic maps ever up to date in all regions. The desultory way in which such observations have been made in the past has resulted in an irrecoverable loss to science.

(*c*) *Observations in ocean depths and atmospheric regions.*—As long as observations are confined to the surface of the earth, the actual distribution of its magnetism and electricity will never be known. The unequivocal solution of the problem demands observations in the waters below and in the air above. The first step consists in devising the proper instruments.

Other problems, such as the correlation of local and regional disturbances of the magnetic needle with geological and physiographic features, and the correlation of magnetic and electric disturbances with meteorological phenomena, might be mentioned.

However, sufficient has been given to reveal the promising field of research which would be opened up to this bureau, in the devising of new and more accurate instrumental means, *in the inauguration of important work in regions unoccupied by any one nation*, in the unification of methods of observation and discussion, in the coordination and correlation of observed data, and in the investigations of problems, not alone of great practical value, but also of great theoretic interest.

Method of Work.

The best means of accomplishing the purposes of the proposed bureau would seem to be through a board of international councilors, whose advice and opinions will govern the inauguration and conduct of a proposed investigation and the appointment of the proper investigators or observers. Much of the observational and experimental work will often be most effectively and economically done by furnishing means to those who have already the problem in hand. The granting of funds to such persons would be made with the assistance of the international board.

Organization.

A director, whose function will consist in the planning and general supervision of the work to be undertaken, assisted by the international council.

An assistant director, who will have immediate charge of the office work and take the place of the director when necessary.

Physicists, observers, computers, and clerical assistance as needed.

Funds Required.

An annual appropriation of $25,000, if it be made available each year, until expended, for a period of ten to fifteen years, would suffice to conduct successfully for the period the work of the bureau as outlined. However, a smaller sum, say $10,000 annually, would be sufficient to cover the expenses involved in the main function of the bureau, viz., that of direction, suggestion, devising of new instruments and methods, and prompt utilization and publication of results. A curtailment of allotment would simply imply abridgment of the experimental and observational work.

Much of the machinery required is already, to some extent, in operation, owing to the influence exerted by the international

journal, "Terrestrial Magnetism and Atmospheric Electricity," which is in touch with every prominent investigator in its special field of inquiry.

Letters Regarding the Project.

[*Mr. O. H. Tittmann, Superintendent of U. S. Coast and Geodetic Survey, to the Carnegie Institution.*]

It is my privilege to forward to the Carnegie Institution a project proposed by Dr. L. A. Bauer for an International or Central Magnetic Research Bureau, for the investigation of problems of the earth's magnetism, of world-wide interest. For want of such an international bureau many of the benefits hoped for from magnetic work, it appears, have not been realized, primarily, because of lack of unification, correlation, and prompt publication of results.

The establishment of a bureau such as proposed has been agitated at various times, and especially of late, by eminent German magneticians, such as Professor Neumayer, Director of the Deutsche Seewarte, Hamburg, and Professor Adolf Schmidt, of Gotha, both leading authorities. Its founding, in connection with an International Research Institution, which I understand is the purpose of the Carnegie Institution, would seem to me most appropriate, and would avoid the prime difficulty likely to be encountered in securing the hearty and harmonious cooperation of all nations, were the Bureau established under the governmental auspices of any one country.

No nation at present can wield a wider influence in magnetic work than our own, because of the extent of territory under our jurisdiction and because of the fact that all magnetic work is being carried out according to a common plan under one organization—the Coast and Geodetic Survey. This influence would be immeasurably increased by the founding in our country of the proposed International Magnetic Bureau. As evidence of the hearty cooperation of all nations that it is possible for us to secure, I desire to call your attention to the success achieved in its work and purposes by the international journal, "Terrestrial Magnetism and Atmospheric Electricity," founded in this country.

The plan proposed is, in brief, to employ the necessary number of persons at the main office at Washington, and to conduct research work by granting funds to the best qualified persons in any country,

the results of such research to be published under the name of the Carnegie Institution, the Institution to be aided in all its work by an International Council.

It is earnestly hoped that this opportunity for establishing such a Bureau in the United States will not be missed, and that sufficient funds may be granted to at least set the project afoot, just now when its need, owing to the marked renewed activity in magnetic work, is most keenly felt. As it would be necessary to begin the work at once, temporary quarters for the bureau could be established in the Division of Terrestrial Magnetism of the Coast and Geodetic Survey, and much of the work of direction of the international work be done by the Chief of that Division, in connection with his regular duties.

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WASHINGTON, D. C., *January 27, 1902.*

[*Professor G. Neumayer, Director of German Naval Observatory, Hamburg, to Mr. Bauer.*]

[Translation.]

I take pleasure in acknowledging receipt of your esteemed favor of the 12th ult., enclosing a copy of the Plan for a Proposed International Magnetic Bureau of the Carnegie Institution, and to inform you that I have read the same with very great interest. I am of opinion that if this plan reaches its fulfilment, it is the most important step ever taken for the development of our knowledge of the earth's magnetism.

The thought which underlies this plan must appeal to every one who has ever been engaged in geomagnetic investigations. In no other branch of geophysics is it more essential to extend the inquiries over the entire earth. Magnetic research, to be successful, requires the cooperation of the most competent investigators of all countries.

As you know, I have occupied myself with the exhaustive collection of magnetic results and with their discussion, and it may therefore not be amiss for me to express my opinion regarding the possibility of success in this line of inquiry without the working together of investigators over the entire globe. Only by international cooperation, as is successfully done in the case of the geodetic and astronomical sciences, is it possible to prevent useless efforts and regrettable errors.

At the "Naturforscher Versammlung" in Hamburg last September (1901), I presented for Professor Schmidt, of Gotha, a plan agreed upon by us for the establishment of an institute for the discussion of geomagnetic results. To be sure, we had in contemplation only an institute for Germany. However, it was also the intention to include in its scope world-wide investigations. Your plan, however, as embraced in your proposition, is far more comprehensive and promises the greater success in case it should be carried out. Through correspondence with Professor Schmidt, I learn that your plan has his indorsement.

The salient points in your plan have been so well thought out, and have so thoroughly the impress of a truly international cooperation in terrestrial and cosmical magnetic investigations, inclusive of atmospheric electricity, that at present I am unable to add anything in the way of suggestion.

Permit me, therefore, in concluding, to express the hope that your plan may meet with success, so that at last we may reach the goal and be able to penetrate more successfully the mantle of darkness still enveloping the phenomena of the earth's magnetism and electricity, thus adding one more to the already notable scientific achievements of the American nation.

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I assure you that it will always be a pleasure to me to assist you to the best of my ability in carrying out the proposed plan.

HAMBURG, *February 11, 1902.*

[*Professor E. Mascart, Director of Bureau Central Météorologique, to Mr. Bauer.*]

[Translation.]

The project which you had the kindness to communicate to me in your letter of January 13 seems to me to be of very great scientific value. If it were possible to secure a participation in Mr. Carnegie's foundation, a first class piece of work could be created.

The profound knowledge of the distribution and variation of the earth's magnetism all over the globe would, besides its evident service to navigation, not fail to contribute to the progress of several other sciences, especially to that of geology, electricity of the atmosphere, and even to astronomy, on account of the still unknown influence of the changes of the surface of the sun.

The science of terrestrial magnetism is, by its nature, essentially international, for it can be treated effectively only by the cooperation of observers of all nations, stationed on land and sea.

The erection of an international bureau of the kind proposed, in the United States, would give to these investigations a mighty impulse.

The program you have worked out seems to be very well prepared in its general outlines.

* * * * * * *

I shall speak about this matter before the Bureau of Longitudes, which occupied itself with this question some time ago, and I shall communicate to you the result of the discussion of this subject.

PARIS, *January 30, 1902.*

[*General L. Bassot, President of the Bureau des Longitudes, to Mr. Bauer.*]

[Translation.]

The Bureau of Longitudes has recently been informed by M. Mascart of the project for the organization of an International Magnetic Bureau of the Carnegie Institution. Our association would see with great satisfaction the realization of this project, which concerns terrestrial and solar physics in as high a degree as it does navigation.

The "Bureau des Longitudes," ever since it was founded, has always seconded, as far as its feeble resources allowed, all efforts which would tend to an increase of our knowledge of terrestrial magnetism.

* * * * * * *

I have the honor to send you the first pages of a general report, now in course of print, which will appear in the "Annals of the Bureau des Longitudes." You will see that the bureau has taken an initiative analogous to that which is proposed to the Carnegie Institution.

It was of the opinion, as you are, that in an enterprise of such proportions one isolated nation would be powerless to bring together, in a sufficiently short time, the elements for a magnetic chart of the globe; it has also made an appeal to the magnetic and meteorological observatories of all nations.

A perusal of this document will show you that the "Bureau des Longitudes de France" is especially prepared, by its methods, its

instructions, and the observations already collected, to second the realization of your project. It is gratified that its efforts have been appreciated and would be happy to see one of its members officially associated with the organization of this important international undertaking.

PARIS, *March 24, 1902.*

[*Professor A. Schuster, Director of Physical Laboratory, Owens College, Manchester, to the Carnegie Institution.*]

I have seen a proposed scheme for an international magnetic bureau, on which I should like to make the following observations:

I believe that no material progress of terrestrial magnetism is possible until our knowledge of the magnetic constants of the great ocean basins, especially the Pacific, have been determined more accurately than they are at present. There is reason to believe that these constants may be affected by considerable systematic errors. It is possible that these errors have crept in by paying too much attention to measurements made on islands and along the sea coast. What is wanted is more numerous and more accurate observations on the sea itself. I have had occasion recently to consider this matter very carefully, and I have come to the conclusion that the observations that are going to be made in the Arctic and Antarctic regions will be of very little use to us until we can supplement them by measurement in other portions of the ocean. It would be most useful, to my mind, to make a complete survey round the world of two circles of latitude, one in the northern and one in the southern hemisphere, say 50° N. and 40° S. of two circles of longitude, say 150° E and 100° W., taking them as far north and south as can be done without much trouble. As regards reduction of observations, there can also be no doubt that private enterprise is no longer capable of dealing with it. Anybody who has not a staff of computers at his disposal is at present incapable of working out any ideas he may have.

The problems which might be worked out are all of very considerable scientific importance. Whether they are also of practical importance is not possible to affirm, but such practical utility is by no means excluded. The other investigations mentioned in the scheme are also of importance.

MANCHESTER, *January 26, 1902.*

[*Professor W. von Bezold, Director of the Prussian Meteorological Institute, to Mr. Bauer.*]

[Translation.]

Your kind favor of the 13th ultimo [enclosing copy of plan of proposed International Magnetic Bureau] gave me very great pleasure. I have always had the feeling that it is comparatively easy to solicit funds for expeditions and similar undertakings designed to collect scientific material, whereas it is very difficult to obtain means or the necessary scientific aid for the discussion and utilization of the data collected.

The difficulties and dangers to be overcome in expeditions evoke energetic young investigators and easily arouse interest in wide circles, while the onerous discussion of the collected material requiring tireless application and more penetrating insight is not valued in an equal degree ; and yet it is the critical discussion of the observations which furnish the actual results of the expedition and make the real contribution to science. This is especially true of magnetic investigations. For the establishment and maintenance of magnetic observatories, and especially for exploring expeditions, large means have been available. The reduction and discussion of observations, however, have been made only incompletely.

For limited regions and for rather restricted purposes—*e. g.*, in the case of magnetic surveys of countries—most gratifying contributions have been made, but for all problems embracing the entire earth, there are most keenly felt gaps in our knowledge. Thus, for example, the immense material gathered by the International Polar Expeditions of 1882–'83 has been utilized only to a very small degree, and so also in the case of the present international work, conducted in cooperation with the Antarctic Expeditions, whereby observations *en masse* will be piled up. It cannot be seen at present how the prompt utilization and publication of the results is going to be accomplished. To be sure, Professor Adolf Schmidt, of Gotha, is at present engaged, with the aid of grants from the Prussian Academy of Sciences, to cover some of the above mentioned gaps ; however, the means at his disposal are altogether too inadequate for the accomplishment of anything very noteworthy. I should therefore hail with delight, as in the interest of science, if a part of the most generous gift of Mr. Carnegie could be devoted to further magnetic investigation.

* * * * * * *

Above all, however, does it seem to me to be important to submit to a critical and comprehensive discussion the immense pile of observational data. This is all the more necessary because in recent times the obtaining of accurate data, owing to the advent of the electric car lines, is getting more and more difficult. Then, first, shall we reap the real benefit of the time, labor, and cost spent in the accumulation of observations.

BERLIN, *January 26, 1902.*

[*Professors J. Elster and H. Geitel to the Carnegie Institution.*]

[Translation.]

Professor L. A. Bauer has submitted to us for our consideration the plan which he proposes for an International Magnetic Bureau of the Carnegie Institution.

With the earnest hope that this proposal may meet with your approval, we beg leave to suggest that it would be in full harmony with the proposed plan to combine with the organization of international magnetic work also the inauguration of observations pertaining to the electric condition of the earth and of the atmosphere, even though this at present may be possible only to a limited extent.

As the principal electric problems, we might name the determination of the strength of the earth's electric field and of the electric conductivity of the atmosphere (the so-called dissipation of electricity), and the investigation of earth currents and the aurora.

Since these matters have been investigated only within comparatively recent times, the methods of observation and of reduction and the theoretical utilization of the results are as yet very imperfect. Nevertheless, there is reason to hope that, even with the present means, relationships between the electric phenomena of the atmosphere and the earth's magnetic phenomena can be disclosed.

At comparatively small cost for instrumental means and without adding very much to the work of the observer it would be possible, in our opinion, to institute systematic measurements of the electric intensity of the earth's field and of the conductivity of the atmosphere at a few magnetic observatories as widely distributed as possible. A few years' results at these places would then show whether it would be desirable to increase the number of stations or expand the work in other directions.

WOLFENBÜTTEL, *January 26, 1902.*

ARCHEOLOGICAL INVESTIGATIONS IN GREECE AND ASIA MINOR

REPORT BY T. D. SEYMOUR.

CONTENTS.

ITINERARY.

Bearing in mind your commission to inquire and report with regard to excavations near the Mediterranean sea, I spent the months of April and May of this year in Greece, among the islands of the Ægean archipelago, and in Asia Minor. On my way to Greece I visited the excavations at Pompeii, with which I was already fairly familiar. On my arrival in Greece I visited the ruins at Eleusis and heard lectures by Dörpfeld, the distinguished head of the Athenian branch of the German Archeological Institute, and by Wilhelm, the accomplished head of the Athenian station of the Austrian Archeological Institute, before the ruins on and about the Acropolis and near the harbors of Athens. I then made three tours

with Dörpfeld, who has lived in Greece since 1878 and during this period has done more than all other scholars together for the advancement of knowledge of classical architecture and Athenian topography. In Asia Minor I was with Professor Richardson, who has been for ten years director of the American School of Classical Studies at Athens.

Since coming to England I have spent more than a month in the libraries of the British Museum, reading reports of explorations and excavations in lands "near the Mediterranean." In the course of my journeys in southern and northern Europe, I have been brought into close relations with several archæologists of distinction, including some who have been engaged in excavations and other explorations, and I have used my best endeavors to secure from them, as from all other sources, the information which you desire.

On the first tour with Dr. Dörpfeld our party visited the excavations of the American School at Athens, both at Corinth and at the Argive Heræum, those of Dr. Schliemann at Tiryns and at Mycenæ, of the Greek Archeological Society at Epidaurus, of Vollgraff at Argos; then, passing to Arcadia, we saw the results of the work of the Germans and the French at Tegea and of the British School at Megalopolis; then, crossing Arcadia, we observed the excavations of the Greeks at Lycosura. Thence we went to Ithome and to Olympia, where we spent three days; thence to Leucas (which Dörpfeld holds to be the Homeric Ithaca), to the classical Ithaca, and to Delphi, where the excavations of the French were carefully inspected. On this first expedition Dr. Dörpfeld had a party of about forty, mostly philologists, but several specialists in archeology. The most noted of the party were professor Diels, of the University of Berlin, and Professor Förster, of Breslau. Dörpfeld lectured from two to five hours every day, expounding the ruins with great care, so arranging his lectures as to touch on almost every topic of ancient architecture, though the order of the discussion was determined by the geographical situation of the ruins, the prehistoric palace of Tiryns and the Argive Heræum being followed by the visit to the sanctuary of Asclepius at Epidaurus, and this by the palace, fortress, and tombs of Mycenæ, etc. The company comprised so many scholars of special attainments that informal discussions often brought out details additional to the lectures. Special trains were in readiness whenever it was desirable and were stopped whenever Dörpfeld wished to show a particular object or view. For the trip to Leucas, Ithaca, and Delphi a special steamer was char-

tered, which should take the exact route desired and should stop according to the will of the head of the party. This first trip occupied fourteen days.

The second tour covered twelve days, and was primarily for a visit to the islands of the Ægean, but provided also for visits to Poros, in Peloponnesus, and to Sunium, Rhamnus, and Marathon, on the Attic coast, which could not be reached so easily from Athens by land. The specially chartered steamer stopped at Eretria, on Eubœa, at least once, and in some cases twice; at Andros, Tenos, Myconos, Delos, Syra, Paros, Naxos, Thera, and Melos, and at five stations on the shore of Crete—Heraclion (for Cnossus), Gurnià, Palæocastro, Phæstus, and Agia Triada. As on the trip through Peloponnesus, Dr. Dörpfeld lectured regularly, sometimes on the boat in preparation for the visit, as well as in the presence of the ruins, and in addition we had on Crete the hospitality and expositions, at Cnossus, of Arthur Evans, the discoverer; at Gurnià, of Miss Boyd; at Palæocastro, of Mr. Bosanquet, of the British School at Athens, and at Phæstus and Agia Triada, of the Italian excavators Parabeni and Pernier, Halbherr, the chief excavator, being ill. This second party was constituted much like the first, but was rather larger, and comprised more dilettanti. The steamer could accommodate more persons than could find horses and mules for the ride across Arcadia, and the life was less strenuous than in Peloponnesus, although the physical exertions required in the visits to some of the islands were not slight.

The third expedition with Dr. Dörpfeld was to Troy, setting out shortly after the return from the visit to the islands of the Archipelago. To Dörpfeld is due all the scientific results of Schliemann's excavations on the site of Troy—indeed, Schliemann died just before the Homeric city was recognized—and here, as at Olympia, every stone was familiar to him. One morning he lectured more than four hours without interruption. At Troy we remained for three days, listening to lectures for at least ten hours, but having some time free for our own independent observations; and from Troy we made an excursion to the heights of Bunarbashi, which before Schliemann's excavations was generally accepted as the site of Homeric Troy. The party to Troy numbered twenty five, being necessarily smaller than either of the others for lack of accommodation. Some of us were quartered in Schliemann's old barracks, near the citadel, but the younger members of the party were obliged to sleep in a Turkish village nearly a mile distant.

The three trips with Dörpfeld were planned to include every important site on which excavations have been made, either on the mainland of Greece or on the Greek islands, with the exception of Attica, which had been visited previously. Architectural and general archaological importance prevailed over historical interest or poetical associations. Thus the party was not taken to Sparta, nor to Thermopylæ, or the Vale of Tempe, where only general impressions were to be gained from a brief stay. The call at Marathon was the only exception to this rule, and this lay directly on the route, in passing from Eretria along the Attic coast.

In Asia Minor, our little party of four, by dint of vigorous exertions, and favored by excellent weather, accomplished in two weeks more than I had thought possible in the limited time at our disposal. Here, too, we visited almost every important site of past excavations: the British and Austrian excavations at Ephesus, the German excavations at Pergamon, Priene, Magnesia on the Mæander, and Miletus, and the French excavations at Didymi, as well as the less important explorations at Sardis and at Magnesia on the Hermus. At Pergamon, Priene, and Miletus we were guests in the houses built for the use of the German excavators, and had the use of their plans and reports for the elucidation of the ruins. We visited also the extensive ruins of Hierapolis and Laodicea, a trifle more than 100 miles from Smyrna, and were also at Thyateira and Philadelpheia, though we had no opportunity there to search for and make examination of any ruins. Thus we traversed a considerable part of each of the four great plains of western Asia Minor, in addition to the Troad, and saw the sites of the Seven Churches of Asia, and passed along the coast in a small Turkish steamer—so near as to give us a distinct view of the country from the west. Reluctantly we abandoned the plan of visiting the scene of the American excavations of Assos; this place is not easily reached, and Professor Richardson was obliged to leave for America.

Thus in the two months of my stay in Greek lands I had under the most favorable circumstances such a comprehensive view of the archeological excavations which have been made in those lands within the last forty years as few before me have had in the same length of time.

The Field Assigned as Limited by Circumstances.

The broad general field which you suggested for my examination receives from circumstances important delimitations. France and

Spain, in general, may be left to explore the remains of antiquity within their own borders, and the feeling is general that Algiers and Tunis are also the proper field of France, which is officially doing much for their exploration, and doing this far better than a foreign expedition, because of the convenient cooperation of civil and military forces in a region which is somewhat unsettled. Danish archeologists desired three years ago to conduct excavations in the Cyrenaica, which was only superficially explored by the English—Smith and Porcher—forty years ago; but conditions were found to be unfavorable, having changed for the worse since 1865, when they were, to say the least, extremely difficult. I have heard of plans made by those who are interested in Roman archeology for excavations on the sites of Roman towns in Africa, but I have no exact knowledge of them.

Egypt.—In Egypt the work of exploration is being done very well, and with sufficient rapidity, chiefly by two expeditions—one from America, with funds supplied by Mrs. Hearst, of California, well directed by Dr. Reisner, who is assisted by Mr. Lythagœ, and the other that of the Egypt Exploration Fund, which receives about half its support from America. This Egypt Exploration Fund has had, for more than a score of years, Dr. Flinders Petrie as the chief director of its work, and about fourteen years ago it instituted a much needed Archeological Survey of Egypt, which has published twelve annual volumes by Newberry, Griffiths, and Davies; and half a dozen years ago the fund established a Græco-Roman branch which has published five volumes of high importance, and is said to have on hand a mass of fragments of papyri which weighs tons rather than pounds. Dr. Petrie in the course of the last twenty two years has done more than any one else ever did in the field for the excavation of Egyptian monuments. In addition to these two principal expeditions, the Germans are exploring in a limited way, with an annual appropriation of about $2,500; the French have recently dug a little with or through Arab explorers, but with comparatively unimportant results; and for the next five years the University of Chicago plans to have a small expedition in Egypt, under the direction of Professor Breasted. If Alexandria, a Greek city on Egyptian soil, is considered apart, as it well may be, we may note that a commission of the Society for the Promotion of Hellenic Studies, after a careful examination, reports that excavations there would be very expensive, and do not promise to be remunerative.

Egypt is not to prove an inexhaustible mine of antiquities, as

many seem to think. An experienced official of the Museum at Cairo believes that the great sites and fields of exploration in that land will be virtually exhausted within fifteen years, and that later explorers will have only a gleaning from the harvest. A more cautious scholar thinks that the opportunities for exploration in Egypt may continue for twenty years yet, except the supply of classical papyri, which is likely to be exhausted earlier; but the work of exploration is so well done now and is so largely under American influence that no reason exists for another special expedition to Egypt. A multiplication of agencies in so limited a field is not desirable, and no new leader would accomplish so much with the same means as the present explorers.

Turkey.—The Turkish government theoretically approves of archeological explorations in its territory, and allows them under certain conditions. Naturally, the consent of the owner of the land on which excavations are to be made must be secured, a government inspector must be paid, etc.; but these are mere matters of detail. Practically, however, the necessary firman for excavations is often delayed. Every request for such permission must receive the approval of the Sultan, and since the supreme head of the Turkish Empire allows little authority to his ministers of state and holds in his own hands all the reins of the chariot of government, so that many matters of importance to the realm are left without attention, no one can feel surprised that petitions which interest no Turk are often and long postponed. An American, Dr. Banks, recently has had action on a request for permission to conduct excavations delayed for more than two years (I think, for five years) on one pretext or another; one site proved not to be available in the opinion of the government, because the neighboring tribes of Arabs were unruly and might cause trouble; another site was not available because part of it was occupied by Turkish graves; and similar excuses are found without difficulty by Turkish officials. But in the past, English and Americans have been allowed to make excavations in the Turkish Empire—witness the American excavations at Assos and at Nippur, and the British excavations at Koyunjik and at Ephesus; and excavations are in progress at present in Asia Minor under the care of Germans, Austrians, and recently by the French. So, with patience, permission may be secured for any American excavations in that region. Well informed persons believe that Dr. Banks's request was opposed by an influence which would not be exerted so strongly against excavations near the Mediterranean. No longer would it be possible for a for-

eigner to dig as Layard did in Assyria in 1845 and Schliemann at Troy in 1870, and di Cesnola on Cyprus, also in 1870, without formal permission from the Porte. The official eye is watchful of excavations. The Turkish law no longer allows a general permission for archeological excavations; the firman is granted for only two years, and may be withdrawn if the excavation is not begun within three months; it is for a single definite place, no more than ten square kilometers in area; and the Minister of Public Instruction may order at any time the suspension of the excavations. Practically, however, when a firman of this kind has once been granted, it is renewed with little difficulty. Doubtless the Porte has been more ready to grant special privileges to the Germans because of the intimate relations at present of the two empires.

Syria.—In Syria two towns near Tyre are reported to show indications of relations between the Carthaginians and later Tyrians or of old Phœnician settlements. These might afford information greatly desired just now. The discovery of a wealthy and artistic civilization in Crete, of the second millennium B. C., which had close relations with Egypt and yet was not dominated by it, rouses special interest in the inquiry with regard to the relations between Crete and the western shore of the Mediterranean. Did the Philistines go to their later homes from Crete, as some have thought, or did they from Syria influence the island? The Rev. Dr. Eddy, who has spent his life near Sidon and has kept in touch with archeological work, recommends the towns referred to, and thinks also that $5,000 or $6,000 expended in excavations on the site of Dan would be very remunerative in results. His opinion is the more valuable since he is perhaps the chief adviser of the natives in their archeological finds.

A much more magnificent undertaking in Syria would be the exploration of the site of Antioch, on the Orontes, the most important of the cities founded by Seleucus Nicator, 300 B. C., in honor of the victory at Ipsus and named for his father, Antiochus—third in importance of the cities of the Roman Empire, being next to Rome and Alexandria, a city which played a great part in the history of christendom, where "the disciples were first called Christians," the patriarch of Antioch ranking with those of Rome and Constantinople, a city about which fierce battles raged during the Crusades. Antioch was the metropolis of the Orient, but it has been shaken frequently by earthquakes since its early ages. Ten times within the first six centuries it is said to have suffered greatly from this cause. The shocks of 457–458 and 526–528 A. D. are

said to have been particularly destructive. The Emperor Justinian rebuilt much of the town, but confined it in closer bounds. It is his walls that are preserved in ruins. They are said to be traced along a circuit of about 4 miles. In the fifteenth century the modern town had only 300 houses. Now there are said to be 5,000 to 10,000 inhabitants, but these occupy only a small part of the ancient enclosure. Thus excavations can be made there with slight expense for the surface of the ground. With Antioch should be included for exploration the neighboring Daphne, only 5 miles distant, where stood a great temple of Apollo and which was a favorite retreat of the Seleucids and the Romans. "For the architecture of but few cities of the world have we such connected reports for a period of 800 years," according to Professor Förster, of Breslau, who has made the most careful study of the ruins and who is most enthusiastic in his recommendation of this as a place for excavations. He would be glad to give to explorers any assistance within his power. His recommendation is seconded by Freiherr Hiller von Gärtringen, who has himself gained deserved note as an explorer and excavator. These excavations would cost not less than $50,000. They would throw light not only on the life of the Seleucid time, but also on the history of the Roman Empire.

Asia Minor.—In Asia Minor, in the course of the last quarter of a century, much has been done in the way of exploration, our country having an honorable part in this because of the work of Professor Sterrett, the results of whose labors fill the second and third volumes of the Papers of the American School at Athens. Professor Ramsay, of Aberdeen, the highest authority on the geography and history of Asia Minor, says in one of his books that he has had occasion to correct the work of many of his predecessors, but that when he has followed Professor Sterrett he has found but a small gleaning left to him after Sterrett's harvest. But the work of exploration in Asia Minor has been too sporadic to accomplish what is most needed, and often a single traveler has been over a route where at least three specialists were required. A party of three explorers—an epigraphist, an architect, and a topographer, with some knowledge of geology—who would supplement the work that has been done, would perform an important work for science. Even after the travels of Heberdey and Wilhelm, I am assured that very much remains to be done for the exploration of southeastern Asia Minor. Many ruins there deserve careful inspection and measurement, and possibly some might be found well worthy of excavation. Professor Sterrett is so

bound by his duties at Cornell University that he could not be expected to retain the conduct of such an expedition for two or three years, but he might be willing to take the leadership for a year, until his associates were thoroughly trained to the work. This work of exploration at present seems to me on the whole more important than the excavation of any known site in Asia Minor, although I shall go on to mention two attractive sites for excavation.

Excavations in Asia Minor have been chiefly of Hellenistic rather than Hellenic sites, and little of an early time has been brought to light. The American work at Assos, next to the great work at Troy, is the chief exception. On the other great sites of excavation little has been found of the best Greek period, to say nothing of the oldest Greek period of colonization. Apparently the earliest sites of some of these towns have not as yet been found, while Pergamon and Priene, of course, had all their glory in the later period. These explorations, then, have been disappointing in their results as regards the art and history of Greece at the time of its greatness, but they have rendered an important service in throwing unexpected light on the relations between late Greek and early Roman culture. Much that had been supposed to be of Roman development is now found to be of Greek origin in art, and particularly in architecture.

Of unexcavated sites in western Asia Minor, two seem easily pre-eminent—Hierapolis and Laodicea. These lie near together, about 100 miles from Smyrna, but on the Ottoman railway, so as to be easy of access. Both are superbly situated. Each is virtually deserted, Laodicea having no inhabitants, but bearing a modest crop of grain, and Hierapolis being haunted rather than occupied by a small band of gipsies. Each has remains of two theaters, baths, aqueducts, early Christian churches, and the like. It will be remembered that St. Paul in his letter (IV, 3) to the Church of Colossæ, which is only 9 miles from Laodicea, refers to the work of Epaphras also at Hierapolis and Laodicea, which proves that these were early homes of Christian churches. Hierapolis was founded about 200 B. C., on a high bluff which commands the plain between the Lycus and the Mæander and the great road from Sardis to the East—the road along which Xerxes led his forces in 481 B. C. and by which the younger Cyrus "went up" against his brother, according to the narrative of Xenophon, at the close of the same century. It was the birthplace of the philosopher Epictetus. The Apostle Philip and his daughter died there. Papias was bishop there in the second century of our era. It was the rival of Laodicea,

and connected politically with Pergamum, being "the center of native feeling, of Phrygian nationality in the valley." Inscriptions have been found there in large numbers; 363 are published by Humann. It was destroyed by an earthquake in the first century of our era, but rebuilt, as is supposed, by the help of the Roman emperors. The excavation of the entire site of Hierapolis is rendered impracticable by the thick deposits of limestone which have been left on the southern half of the ruins by springs which are heavily charged with minerals. These deposits since the destruction of the city are often 6 feet or more in thickness. Indeed, the bluff is a remarkable natural phenomenon. On either side of the ancient site is a petrified Niagara formed by these springs. The great theater, however, is well preserved, and lies so high that no limestone deposit has been made about it, and other important ruins also are free for excavation. The most thorough survey which Hierapolis has yet received was by a small party which remained only a fortnight, and hardly had time to turn over stones in order to see if these bore inscriptions.

Laodicea presents no such difficulties as the neighboring Hierapolis. The form of the long mounds which border the principal streets indicates that the rows of houses which lie beneath are concealed by no great depth of earth. This city was somewhat older than Hierapolis, being ascribed to Antiochus II and named for his wife Laodice. So far as appears, no archeological excavations have been made there; but the ruins were plundered somewhat for the construction of the Ottoman railway. An ancient Greek inscription from Laodicea lies face upward as a block in the platform of the neighboring railway station (Gondjeli).

Greece.—The Greek government allows responsible societies or institutions to conduct archeological excavations under government surveillance, recognizing the reasonableness of the desire to have the antiquities uncovered and not having money enough to undertake all of this work. The Greek governmental oversight is in no respect vexatious, but is designed only to secure a strict observance of the laws with regard to antiquities. Private persons are allowed to conduct excavations in Greece only in the name of some responsible institution; otherwise the government so limits the authority of the excavator as to make the work an official excavation at the expense of the individual. No man may even make archeological investigations on his own land without the permission

of the ephory, and the ephor will direct the work, determining even the number of laborers to be employed. Clearly, this law is intended to discourage irresponsible explorations, and it is entirely reasonable. Many foreigners would be glad now, as a century ago, to expend a little money in turning up the soil of Greece, although without any real scientific interest and not fitted, either by nature or training, for the conduct of scientific explorations.

Crete.—The Cretan law with regard to excavations is similar to that of Greece, but the government has granted permission to excavate to scholars whose position is assured. Mr. Arthur Evans's extraordinary skill is well known to the Cretan authorities, and after our countrywoman Miss Boyd's excavations at Kavusi in the name of the American School of Athens she could have had no difficulty in securing permission to dig on her own account, though I believe her present excavations are for the American Exploration Society of Philadelphia.

Italy.—The Italian government declines to permit foreigners to conduct archeological excavations on its soil, even in the way of mere superficial exploration. In the past such work has been allowed at times (as it was, freely, in the sixteenth to the eighteenth centuries), and hopes have been entertained that under suitable restrictions such permission might be again granted ; but as yet these hopes have not been fulfilled, and the policy of the government seems fixed, although attention has been called to the inconsistency of refusing this privilege to foreigners, while it is granted to Italians who are believed to desire the permission not because of their interest in archeology, but simply for commercial reasons. But if, as has happened in recent years, a foreigner wishes to have some house removed which obstructs and covers important ruins, he can not conduct the work himself, but must give the money to the Italian government, merely expressing the desire that it should be used in a certain way.

Cyprus.—Since Cyprus came under the power of Great Britain, the ancient sites on that island are practically reserved for British archeologists. Small excavations have been made, it is true, by both French and Germans, but only by way of exception ; in at least one of these cases the privilege was requested as a personal favor by an "exalted personage." Under ordinary circumstances the British Foreign Office would not grant to foreigners permission to dig for antiquities on that island.

Past Excavations.

Greece.—Thirty years ago no archeological excavations worthy of the name, according to present ideas, had been undertaken in Greece. The best had been the uncovering of the great theater of Dionysus at Athens, but this was not completed. The soil had been scratched a little on the Athenian Pnyx, a little rubbish had been removed from about the Parthenon, the French had spent six weeks in removing the earth from the temple of Zeus at Olympia, less time had been spent in clearing the sanctuary at Eleusis, many graves had been opened by unauthorized persons, and a few of the tombs in the Ceramicus had been opened by authority. Now, however, the principal and most promising sites have been subjected to a careful archeological search, and the list of places where important investigations of this kind have been made would read like a catalogue of the chief political, commercial, and religious centers of Hellas. Dodona, the earliest seat of the worship of Zeus in Greece, was discovered by Carapanos and excavated by him in 1875. Olympia, the seat of a famous oracle and of still more famous athletic contests—the chief common meeting ground of Greeks of all tribes, whether their homes were in Libya, Sicily, Macedonia, or Hellas proper—was excavated by the German Empire in 1875–'81 at a cost of about $200,000. Only in May of this year the French handed over to the Greeks the results of their excavations on the site of the sanctuary of the Pythian Apollo at Delphi, excavations which in their final stage lasted for ten years and which are said to have cost about what was expended for the similar work at Olympia. The site of the worship of Apollo at Delos was attempted by the French thirty years ago and the work was continued later, but was not completed according to modern standards, and it is to be resumed by them within a few months, an American, the Duc de Loubat, furnishing the money needed for this work. The ancient and honored shrine of Demeter at Eleusis, the seat of the Eleusinian mysteries, and the extensive sanctuary of Asclepius at Epidaurus, the chief seat of worship of the Greek god of healing, with temple, theater, stadium, and many other accompaniments of a health resort, has been excavated by the Greeks themselves rather gradually. The earliest and chief seat of the worship of Hera in Greece, the Argive Heræum, including not only the temples but also the neighboring porticoes and other buildings, was excavated by the American School of

Athens, assisted by the Archæological Institute of America, from 1889 to 1893. The temple of Apollo at Bassæ in Arcadia, from which the frieze was brought in 1812 to the British Museum, lay several miles from any town, and no indications have been found near it of any such complex of buildings as at Epidaurus or the Argive Heræum, but the ground immediately about the temple has been searched by the Greek Archæological Society. The French have investigated the site of the temple of Ptoan Apollo in Bœotia. The American School of Athens had the honor of uncovering the remains of what seems to have been the earliest site of the worship of Dionysus in central and southern Greece—Icaria in Attica, the home of Thespis, who was long considered the mythical founder of the drama, but who has assumed a clearer historical personality since these excavations. The ancient seats of wealth, culture, and power at Mycenæ and Tiryns were excavated by Dr. Schliemann. He dug also at Orchomenos, in Bœotia, where the work was resumed last spring by Professor Furtwängler, of Munich, who found interesting objects of a very early age, including specimens of the same early (non Greek, non Phœnician, non Egyptian, non Assyrian, non Cyprian) writing which covers thousands of tablets discovered in the palace at Cnossus, in Crete. The temple of Lycosura and its surroundings, which was called by ancient tradition the oldest town of Peloponnesus, has been excavated by the Greeks. The site of Corinth is in process of excavation by the American School.

At Athens more or less archeological digging has been done since it became the capital of the new Greek kingdom, from 1834 on. Gradually the summit of the Acropolis has been cleared of rubbish and from buildings of the Frankish and Turkish times, and the important structures on its slope—the Theater, the Odeum of Herodes Atticus, the Asclepieum and the Stoa of Eumenes—have been either brought to view, as the Theater and the Asclepieum, or have been cleared, as the Odeum. In the city of Athens systematic excavations have been difficult or impossible. When the seat of government was fixed there, seventy years ago, the site of the ancient city was covered by the Turkish town. Low, mean houses these were, and little money would have been needed for the purchase of a large district; but the Greeks then had *no* money to expend for archeological purposes. If modern Athens had been built at the Piræus, as some urged on other accounts, the solution of the archeological problem would have been easy; but the value of the land in the city has increased about as rapidly as the Greek government's means for

excavations. A peculiar and unfortunate "squatter's law," together with the carelessness of officials, allowed to certain "squatters" rights to land immediately adjoining the Acropolis, which was not occupied when the Greeks returned to Athens and which never should have been used for buildings. These have been dispossessed, but no thorough and systematic examination of the soil of Athens has been practicable.

The American School has conducted excavations at Eretria and Sicyon in addition to doing less important work on half a dozen other sites. The British School has excavated at Megalopolis, the French at Mantinea and Tegea. The Greek Archæological Society has conducted important excavations at Thermon, in Ætolia, the capital of the Ætolian League, and at dozens of other places, the income of the only lottery authorized in Greece being given to this society to be used for excavations and for the care of the ancient monuments. In the year 1902 the Greeks conducted excavations in twelve or thirteen places in their own kingdom, in addition to beginning the work of laying bare the ruins of the great temple of Hera, at Samos, which is still tributary to Turkey, though on a different footing from Chios and Lesbos.

Islands.—The islands of the Ægean sea have not been neglected either. The French have dug on Delos intermittently, and on Amorgos, The British have dug on Melos. The Austrians explored Samothrace. Freiherr Hiller von Gärtringen has excavated what may be called a Greek Pompeii on the island Thera. Early in the nineteenth century (1811) slight excavations were made on Ægina at the time when the sculptures of the temple of Aphæa were removed, and within the last two or three years Bavarian archeologists have carried these excavations further. For two years a Danish society has dug on the island of Rhodes. Other excavations also have been made on these islands, but it would be too long to enumerate all of them here. Even Belgians here have entered the lists of excavators. Englishmen have explored Kos, and one of their countrymen spent several weeks this summer on Karpathos, exploring it, as is believed, with a view to the discovery and excavation of Mycenæan sites.

Crete.—For many years archeologists have looked to Crete for the solution of many of their problems, and occasionally endeavored to explore it. About twelve years ago the Archæological Institute of America organized an expedition for Cretan exploration, but the conditions proved more unfavorable than was anticipated, and the plan was soon abandoned. Naturally, then, when this island was brought

under the protection of the Great Powers, and freed from the vexations and uncertainties of Turkish rule, not a few—among whom Italians, English, and Americans were most prominent—were eager to begin explorations and excavations there. Several Italians had made themselves familiar with the island, and have dug at Gortyna, Phæstus, and Agia Triada, near the southern coast. At Gortyna, a town mentioned by Homer as "well walled," was found eighteen years ago the longest law code which has come down to us from ancient times—from about the middle of the fifth century B. C.—of particular interest because of the recognized high reputation in antiquity of the Cretan laws. This was one of the sites on which our Institution hoped to make excavations ten years ago; but archeological exploration is not easy at this point even now, because of a water-course and mills which involve vested rights. At Phæstus the Italians have found the remains of a magnificent palace of the same period as that at Cnossus—not so extensive, but rather better preserved and of equally impressive proportions. Agia Triada lies only about 3 miles from Phæstus. The excavations there are not completed, but the ruins seem to be those of a nobleman's residence rather than of a palace, in the English sense. Twenty five years ago certain discoveries seemed to indicate the site of the old palace of Minos at Cnossus, 4 or 5 miles from Candia, on the northern side of the island. Dr. Schliemann at one time nearly completed arrangements for the purchase of the land and the conduct of excavations, but some new difficulty arose and the negotiations were broken off; but as soon as the new government was established at Crete, Mr. Arthur Evans—a distinguished son of a distinguished father and the keeper of the Ashmolean Museum—who had long been waiting for this opportunity, with much patience and ingenuity in meeting difficulties and in overcoming them, purchased this land on his own account, and has there made the most brilliant archeological discoveries of the last twenty years. Since the final destruction of the palace, perhaps 300 years B. C., its site has been uninhabited and even untilled, so that in places the ruins were hidden by no more than a foot's depth of earth.

Miss Boyd began her excavations in Crete at Kavusi, on the northern shore, in the name of the American School of Athens, with part of the stipend received by her as fellow of that school. During the last two seasons she has dug at Gurnià, not very far from her former site, receiving her principal support from the "American Exploration Society," but using also her own limited means. She has found the

best example yet known of a "Mycenæan" village, with town chief's house, but no palace; and with a town shrine, the first such to be known of this period, though at Cnossus earlier and later shrines have been found in the palace. At Palæocastro, near the eastern end of the island, Mr. Bosanquet and his associates have a Mycenæan town larger than Gurnià, but of no great splendor, though with very delicate and beautiful pottery. The cave of Dictæan Zeus has been explored by Mr. Hogarth. Less important excavations, like those at Zakro and Præsus, may be passed over here.

Opportunities in Greece.

Although excavations have already been made on the most noted sites and those which promised the surest and richest rewards for investigation, yet in Greece, too, interesting, important, and promising sites remain almost entirely unexplored. I would name Thebes, the Minoan promontory of Megara, Elis, Sparta (including Amyclæ), Gythion (the port of Sparta), and Samikon (near Olympia, on the west coast of Peloponnesus). I will write of these briefly in reverse order.

Samikon is thought by some to have been the western terminus of a "trade route" through Peloponnesus, from Gythion on the east, a thousand years or so before the beginning of our era. Excellent "polygonal" walls lie on it, and no excavations have been made there. Satisfactory trial excavations might be made for $1,000, in my opinion.

Gythion was not only the port for Sparta, but also the port for Amyclæ, the older capital of the Eurotas valley; and if ever a "trade route" crossed Peloponnesus, this must have been the great eastern terminus, whatever was the western. This site is urged for excavation not only by classical archeologists, but also by Mr. Flinders Petrie, who hopes that there will be found objects which will throw light on the relations between Greece and Egypt in early times. Little has been done in the way of excavations at Sparta, partly, no doubt, because of the improbability of finding great ruins. Thucydides says that in ages long after him men will hardly be ready to believe the former power of Sparta, while they will exaggerate that of Athens, judging from the ruins in each case. No one can hope to find a Parthenon or Erechtheum there, and no walls ever existed to be traced now, and no such mass of inscriptions as have been found at Athens was ever known in Sparta; but

many indications may be found of the early life, history, and institutions of the Spartans, and perhaps of their predecessors. Long ago C. O. Müller suggested that the real home of the Pelopids was to be sought at Amyclæ rather than at Mycenæ, and not a few indications point to this conclusion. Near Amyclæ have been found golden cups, ornamented with scenes of the bull hunt, in "Mycenæan" style, of an advanced type of art. Since the Cretan excavations and the discovery of the "Mycenæan" culture there, archeologists are eager to ascertain where this civilization was developed. On the discovery of the palace at Cnossus, many were disposed to regard this as the chief center of the "Mycenæan" art and life, but now some archeologists are disposed to turn their eyes back to Greece as the original home of this civilization, and since Argolis has been explored—at Mycenæ and Tiryns—the chief new light for this question, from Greece proper, must be expected from the region of Amyclæ.

Elis was the chief town of Western Peloponnesus, and almost nothing has been done as yet to explore its site. This name has come first to the lips of two or three archeologists when they were questioned as to the opportunities for excavation in Greece.

The very name of Minoa, at the harbor of Megara, reminds one of Minos and Crete and the Phœnicians. A recent ingenious writer counts this as the beginning of a Phœnician trade route through Bœotia, and, though we may not believe the Phœnicians to have had so strong a footing in Hellas as Bérard's theory implies, this Minoa is indicated as one of the landing points and trading ports of the Phœnicians, and thus as one where remains might be found which would throw light upon the early relations between the east and the west.

Thebes was the center of a large body of myths and poems. In the fourth century B. C., before its destruction by Alexander the Great, it had 30,000 or 40,000 inhabitants. After its restoration it may have had 10,000. At present it has only some 4,000 or 5,000 inhabitants, and occupies but a part of the old Cadmea, and fewer remains of antiquity are left above ground there even than at Sparta. The excavations which have been made there seem to afford perfect confirmation of the supposed myths as to the age and early influence of the city. Confirmation of the story of its connection with Phœnicia remains to be given. On the whole, I am disposed to recommend this site for excavation rather than any other in Greece, particularly since the railway from Athens to Thessaly, which has already

been completed into northern Attica, is to be extended at once to Thebes. This is sure to stimulate the rapid growth of the city as the center of a very fertile plain; the price of land is sure to rise rapidly, and the experience of Athens is likely to be repeated there—houses will be built over the remains of ancient Thebes, and systematic archeological excavations will soon be put out of the question. A further consideration in this connection is that the digging of the navvies for the railway is sure to bring to light much material and many facts which would be of high value when combined with those secured by the archeological explorations.

I should like to write in detail with regard to the American excavations at Corinth. These are the most important archeological excavations in progress at present in Greece, and they have received marks of higher appreciation from archeologists abroad than from the public at home. Those who *complete* this work, naturally, will receive the credit of it, and others would be very glad to continue the explorations there, if the American School is obliged to abandon the site. European archeologists believe the results attained at Corinth to be large in comparison with the sum expended, which to the present time I suppose to be about $15,000, which is only a fifth of that expended at Ephesus, yet with as important scientific results. Excavation is not the chief work of the American School at Athens, and money for this work at Corinth has been secured with difficulty.

Opportunities in Crete.

In Crete, Miss Boyd's work certainly ought not to be allowed to end before she considers it completed. She has achieved important results with small means. She could not have done what she has if she had not paid a large part of her own expenses and found helpers who were ready to pay theirs. At Gurnià, last May, Dr. Dörpfeld spoke highly of her work, but after the party reached the steamer he called the company together and said that he should have spoken more highly of the excavations on the spot if he had not understood that it would be distasteful to Miss Boyd, who was present; her work "could not have been better done." Her explorations at Gurnià are nearly completed. She would be glad, however, I am assured, to make a thorough exploration of the route which passes that town, from the northern to the southern shore of the island. This route is only about 10 miles in length, and seems to have been used in very ancient as well as in modern times; it was used as a

"portage" by the French troops at the time of the recent military occupation of the island by the Great Powers. What support Miss Boyd is likely to receive from the society which has furnished her a small sum of money (I think only $2,000 this past year) for the work of 1901–'03, I do not know. Such explorations can be most economically administered with a larger sum of money to expend.

As for Miss Boyd's personality, I may say that she graduated at Smith College about 12 years ago, and is now instructor in archeology in that institution. In the year 1896–'97 she was a student in the American School at Athens, and was preparing to enter the competitive examination for a fellowship of the school. On the outbreak of the war with Turkey, however, she went with several Athenian ladies as a volunteer nurse to the hospitals of Thessaly, where she attracted much attention because of her unusual "capacity." In a later year she entered the fellowship competition which she had abandoned in the spring of 1897, and was successful. At the expiration of the year of her first fellowship she was appointed Hoppin fellow of the school, with a stipend of $1,000, half of which she used for her first excavations at Kavusi. Her life in Thessaly aided to give her an excellent command of the Modern Greek language, and brought her into touch with the Greek people in a way which has been useful to her in Crete. I should add that her reports of her explorations are admirably clear and methodical.

The suggestion has been made that America might join with Mr. Evans in his work at Cnossus, but he has nearly accomplished his great task. The great palace has been uncovered. It is true that an earlier palace lies in ruins under those of the later palace, and he will run some tunnels next year to learn what he can of the earlier without disturbing the later structures, but his work is substantially completed.

As for fresh sites of excavation in Crete, almost nothing has been done for the exploration of the western end of the island, which we may suppose to have stood in the closest relations to the peoples on the mainland of Greece; but no site there seems to be preeminently attractive, and a careful archeological survey and exploration of the island seems wise before further excavations are made, though such an exploration might discover almost at once a particularly attractive site. A French survey of the island under Ardaillon was planned a year ago, but I have not learned that it has been actually undertaken.

LAWS AS TO EXPORTS OF ANTIQUITIES.

The time is past when private or public museums can be enriched with works of art and curiosities by excavations. These now have to be conducted on a more ideal basis, for the advantage of the science of archeology and not as a commercial speculation on the part of the digger. In retired districts of classical lands some of the inhabitants have had much experience in finding and opening tombs for the sake of the treasures or trifles which were buried with the dead, and think this occupation to be more remunerative than agriculture; but all exportation of antiquities from the lands of ancient culture is now contrary to law. At the opening of the nineteenth century, when Greece was still in the hands of the Turks, Lord Elgin, by gifts and diplomatic arts, secured permission to "draw, model, remove, and excavate" any of the old buildings at Athens, and made large use of the right to *remove*, taking all that he wanted of the sculptures of the Parthenon, the frieze of the temple of Unwinged Victory, one of the Caryatids, a column and a long piece of the frieze of the Erectheum, the statue of Dionysus from the choragic monument of Thrasybulus, etc. Not much later, in 1811, two young Englishmen and two Germans stole the sculptures from the temple of Aphæa on Ægina, which are now the chief ornament of the Glyptothek, in Munich, and in the next year the same party, with one or two more, by a heavy bribe persuaded the Vizier of Peloponnesus to allow them to remove the sculptures from the temple at Bassæ, which were sold to the British Museum for £60,000. Mr. Hamilton Lang conducted excavations for antiquities on Cyprus in 1870 during two months without a firman from the government, and the sculptures are in the British Museum. About the same time General di Cesnola opened hundreds of tombs before he was obliged to seek the permission of the government; then he obtained a firman for the excavations which furnished the treasures for the Metropolitan Museum. Probably under the influence of the Cyprian excavations, attention having been called to the value of antiquities, Dr. Schliemann, in 1871, was obliged to promise to give to the Turks a share of the objects found by him at Troy, and difficulty was made in renewing the firmans. He conveyed away the most splendid of the gold treasures found at Troy, but later gave to the Turkish government $10,000 for its share of this treasure. From 1863 to 1874 Mr. J. T. Wood, an English architect, was engaged in excavations at Ephesus. He was allowed in 1863 to export all antiquities which

he might find, leaving the *duplicates* for the Turkish government; but in 1872 he reports that the Turks were disposed to grant no more firmans for excavations; they would do this work themselves. The Germans, however, were allowed to excavate on the site of Pergamon, beginning in 1878, on condition of giving to the Turkish government one third of the objects discovered, which third they later were allowed to purchase. The Americans were allowed to excavate the site of Assos (1879–1881), giving to the Turks one half of the objects found; and the Austrians were allowed to remove the sculptures from the Heroön of Gjölbashi, in Lycia (1881–1883); but these seem to have been the last firmans for excavation in Asia Minor granted by the Turkish government that include the right of exporting the objects found, or any considerable number of them. Excavations in the interior of Asia seem to be on a slightly different footing.

One of the first acts of the newly established Kingdom of Greece in 1834 was to forbid the exportation of antiquities. The Greeks had been humiliated and exasperated by the removal of the "Elgin marbles," which was followed soon by the abstraction of the sculptures from Ægina and Bassæ, and their government saw clearly that it would be mischievous to allow to pass freely from their country the memorials of their country's nobler past, and that not only professional scholars, but also other visitors, might be drawn to Greece for the sake of seeing its antiquities. The law was far in advance of public sentiment, however, and its influence has not been altogether beneficial. Men of learning and high position, professors in the University of Athens, not only connived at such smuggling, but were believed themselves to be dealers in antiquities to be delivered to the purchaser outside of Greece. I have myself known a man of distinction to amuse himself with outwitting the Greek custom house officials and conveying antiquities out of the country. Discoveries of antiquities, instead of being announced at once, were generally concealed, that the finds might be the more easily carried or sent from Greece. Thus the circumstances of the discovery, often of greater scientific interest than the object in itself, were concealed or passed unnoticed. Not infrequently also objects too large to transport easily in secret, such as statues or stelæ, were broken, and the large number of heads of terra cotta figurines without bodies offered for sale has been explained by the disposition of the finder to save only what he could most easily keep for himself. The mass of antiquities, large objects as well as small, which have reached the

museums of Europe and America from Greece in the course of the last seventy years is sufficient evidence that the Greek law against exportation is often evaded, and the efforts to elevate the public sentiment on the subject have not been very successful. The peasants in general do not yet understand why their government or any one else should care for broken stones or old pottery, and if strangers are disposed to pay a good sum for these trifles, why should they not have them? The Greek senate has recently passed a much stricter law, making every work of antiquity, wherever or however found, in Greece or in Greek waters, the property of the state, if this cares to take it for its museum. In this case the finder is to receive from the Archæological Society half of the value of the article. Every official of the government is bound under heavy penalties to see this law enforced. For dereliction of duty he is liable not only to be deprived of his office, but also to be sentenced to fine and imprisonment for two years. But very recently a party of archeologists visiting Eretria brought away with them vases that, in my opinion, were not only more numerous, but also more valuable, than those in the Eretrian Museum, all purchased within a few feet and almost under the very eye of the soldier on duty as a policeman. Though it is only fair to add that the best vases from Eretria were already in the museum at Athens, yet the letter of the law was flagrantly violated in the sale to the archeologists.

Contrast with Former Spirit.

The Turkish government in 1884 enacted a similar law to that of Greece, absolutely forbidding the exportation of antiquities; but this law, too, is not supported by public sentiment, and the exportation continues, though attended by difficulties. The European or American purchaser is obliged to pay a higher price because of the increased risk to the seller, and the high price in turn encourages illegal digging, and thus leads to archeological loss. The Germans, on renewing their excavations at Pergamon, receive for themselves only the objects that are needed to complete those which they removed to Berlin a score of years ago.

The great Museum of Cairo is already provided with a large supply of the ordinary objects found in Egyptian tombs, but the law forbidding the export of antiquities, except such as are derived from authorized archeological excavations, is so strictly enforced that a prominent dealer in antiquities has recently given up his business, it

being no longer profitable. A movement has been set on foot lately to forbid the export of any antiquities from Egypt, but this law is thought unnecessary, since the present statute virtually allows the Museum at Cairo to select what it wants from the objects found by the explorers.

One of the first acts of the new Cretan government was to enact a law similar to that of Greece, but still more stringent in its provisions. No antiquities may be exported from the island except those that are registered as such, with many formalities, on their exportation. The sentiment of the people on the subject is said to be good. Foreign excavators there pledge their word of honor that they will not convey from the island any of the objects which they find.

In all lands, however, the right of first publication is reserved for the excavator or finder.

Thus all future excavations in classical lands are to be conducted on the ideal basis—not for spoils, but for science. Twenty five years ago the Germans were called sentimentalists for undertaking the excavations at Olympia for the Greeks with the agreement that they should receive for themselves only duplicates and the right to make casts and photographs, and it was thought that they would have but few successors. Ten years later, however, the French proposed the excavations at Delphi without asking even for duplicates, if there should be any such. And the strictness of the Greek law has not checked the desire of foreign nations to take part in this work of uncovering the monuments of the past.

The motive in archeological excavations today is in marked contrast to that which prevailed in former times. Men have always been aware that objects of more or less value were often or generally buried with the dead, frequently, no doubt, in the belief that the spirit of the dead would be able to use or enjoy them in some way, and again often merely as a tribute of affection, just as flowers are laid upon a grave today. Men have been tempted from time immemorial to open tombs and to take whatever they could find there for their own use, though the act and the occupation of a grave robber were despised. Thus in some districts the majority of ancient tombs were opened and plundered in early times—many centuries ago. Of thousands of sarcophagi in Lycia, not one has been discovered in modern times which had not been opened previously. In other lands less of this work had been done in antiquity. A few years ago, according to report, three thousand Bœotian tombs which had not been robbed previously were opened by the country people before

the interference of the government, in the search for terra cotta figurines, which already were bringing a high price in the market, though not so high as at present.

In the fourteenth, fifteenth, and sixteenth centuries of our era, with the new interest in ancient literature was awakened also interest in the monuments of antiquity, particularly in works of ancient sculpture. Much digging was done in the neighborhood of Rome, on the sites of ancient villas, and many statues were brought to light. These were valued chiefly for themselves, however, as works of art, and if (as almost always) they were broken they were "restored" according to the ability, taste, and caprice of their owners. No one shrank from adapting the head of one statue to the body of another, if these could be made to fit by changing one or both of the parts, nor would the owner hesitate by an alteration of attribute to make a muse into an Artemis or a Demeter. This method of dealing with objects of antiquity continued until recent times. They were considered more valuable when "perfect," though the perfection were the result of a "restoration." Early in the nineteenth century the statues from the pediments of the temple of Aphæa on Ægina, having been purchased by the crown prince of Bavaria, were entrusted to the sculptor Thorwaldsen, who "restored" them most carefully and conscientiously, but archeologists now regret that they were "restored" at all. Canova deserves and receives credit for declining to add arms and noses and heads to the "Elgin marbles" from the Parthenon at Athens, when this was suggested. But only a third of a century ago most men of culture were not shocked—only a few archeologists were troubled—at the thought of a "restoration" of the statues found on Cyprus. The proper treatment of broken works of art, I may say parenthetically, has never before been exemplified so well as in the French treatment of the sculptures found in the recent excavations at Delphi, shown in the museum opened there last May: the broken marbles are set up as nearly as possible in their proper positions and relations, with no additions, while near them are placed plaster models with the missing parts restored according to the judgment of the archeologists and artists in charge.

The early expeditions for archeological purposes were insufficiently equipped, and thus desire for objects to fill museums appears as their exclusive aim. Even only thirty or forty years ago the "science of the spade" was in its infancy. Fr. Lenormant published in 1862 the results of his *Recherches archéologiques* at Eleusis two years before; but he has nothing to say except about the inscrip-

tions which he found there. Wood conducted excavations at Ephesus for more than ten years, but not as an archeologist. His aim was to find the great Temple of Artemis, and he cared little for the instruction afforded by the smaller objects which might be found in the course of the excavation. Few of these smaller objects came into his hands. They seem to have been considered a sort of perquisite of the workmen. He had not learned that a small object may be quite as instructive, though not so imposing, as a large object. During a large part of his excavations Wood was busy in his vocation as architect at Smyrna, and his explorations at Ephesus were left entirely in charge of his foreman, who laid not even the slightest claim to archeological knowledge, but was chosen simply for his skill in keeping the laborers at their work. Similarly Schliemann undertook his excavations on the hill of Hissarlik with no archeological preparation or associate. He dug at first simply to prove the truth of his theory that the hill of Hissarlik was the site of Homeric Troy. He put his first great trench 40 feet deep and broad in proportion through the upper part of the hill, exactly as a railway contractor would make similar cutting for his tracks. He had at work 150 men with barrows and carts, but not a single archeologist to watch or to advise. Of course some irreparable damage was done by the destruction of ruins in the upper strata with no adequate observation and record of them. After finding the great treasure of gold at Troy in 1873, and, still more, after discovering the wealth of gold array in the tombs of Mycenæ, Schliemann dug both as a treasure seeker and to prove the truth of his theories, but hardly as an archeologist. He seems to have been disappointed at not finding gold at Tiryns, and to have been disposed to consider his excavations there a failure until scholars of all lands declared his architectural discoveries to be more valuable than a mass of gold. At Mycenæ his interest in the treasures of the tombs was so overpowering that he took small pains to preserve the tombs themselves and allowed a considerable part of the "sacred circle" to be destroyed. But he learned to excavate by excavating, and attained considerable archeological knowledge and skill. To him more than to all other persons is due the extraordinary interest in modern archeological excavations, but his work had no scientific character, in the modern sense, until 1882, when he secured the cooperation of Dr. Dörpfeld. General di Cesnola, too, in digging in Cyprus, had had no more archeological training than Schliemann, and a large part of his work (according to current report) was done for him, like the excavations

of Wood at Ephesus. These men achieved important results, but in considering their work we must bear in mind that they were not archeologists at the beginning; they learned their trade by practicing it—by many costly experiments. Now all this method is changed. In the last excavations on the hill of Hissarlik Dörpfeld employed only one seventh as many common workmen as Schliemann had set at the big trench, and had with him three or four trained archeologists to observe, direct, and study.

In Egyptian explorations pains are taken to allow no scarab to be lost. Of slight "museum value" in itself, any such may supply the clue for the solution of some problem. In the excavations of Flinders Petrie in Egypt last winter, one object considered worth all the rest was a little image of the old king Khufu (Cheops), no longer than a man's hand, which would have escaped notice in excavations like Wood's. Modern archeological excavations are much more expensive than those conducted on the railway contractor's plan, but they are also much more instructive than those that were intended primarily to fill museums. Reinach, one of the most distinguished of French archeologists, in going a few years ago to dig at Myrina, in Asia Minor, where an indefinite number of terra cotta figurines had been found, said expressly that the expedition had as its first object not the finding of figurines, but to learn what was possible of the burials and the ancient life at Myrina. Now that the temptation to dig simply for objects of antiquity is removed by the law which forbids their exportation, archeologists are free to explore simply for the sake of science. Mrs. Hearst, of California, be it said to her praise, has given this direction to the party under Dr. Reisner which is digging at her expense in the soil of Egypt; they are to do what is best for the science of Egyptology, without regard to showy discoveries.

In another respect, too, the archeological excavations of the present differ from those of the past. Fifty years ago no one planned to lay bare the entire site of a sanctuary, and still less that of a town. Even at Pompeii until 1861 the excavators chose what seemed to be a promissing place here, and another there, as if they were picking blackberries in a mountain pasture. There, too, naturally enough, more attention was paid to the objects found than to what might be learned from the position of the objects, and after the antiquities had been removed to the Naples Museum no sufficient care was taken of what had been uncovered, but left, at Pompeii. Naturally, too, Wood's excavations at Ephesus were entirely un-

systematic. In order to find the great temple there he sunk trial pits for six years before he identified the sacred precincts, and dug for three or four months longer before he found the temple itself. More systematic digging would have found the temple sooner. And when he had found the temple, deeply embedded in a marsh, he dumped a considerable part of the earth which he removed from the temple upon the spot where we may believe the great altar to have stood. So the British Museum plans this year to resume those old excavations and to complete them according to modern methods. Similarly, at Delos, the French excavated buildings here and there, leaving between them in many cases spaces of unexcavated ground, with the result that the visitor has no connected view of the sacred precinct of Apollo as a whole. Here, too, because of the lack of a systematic and generous plan at the first, much of the earth has been moved twice, and part of it, I am told, three times. That the French expect to resume these excavations this year I have already stated. The early parties for the conduct of excavations and explorations in many cases were not well equipped with specialists, though these generally were not entirely lacking, as they were in the cases of Schliemann, di Cesnola, and Wood. The French seem to have been the first to send out companies for the purpose of investigating not only the "antiquities" and monuments of the country, but also its topography. The American excavations at Argive Haræum had a trained architect, but such a specialist is needed urgently also for the American excavations at Corinth, and has been needed in a less degree in the minor excavations as at Eretria and Sicyon.

Present Excavations in Greece.

At present excavations on Greek lands are carried on as follows, in addition to several minor excavations of the Greeks themselves:

Corinth: The American School at Athens.
Argos: Mr. Vollgraff, supported by a Hollander.
Tegea: To be resumed by the French.
Pergamon: The German Institute, under Dr. Dörpfeld.
Ephesus: (The Temple of Artemis.) Resumed by the British Museum.
Ephesus: (The City.) The Austrian Institute.
Miletus: The Berlin Museum, under Dr. Schrader.

Crete :

Cnossus : Mr. Arthur Evans.

Palæocastro : The British Society for the Promotion of Hellenic Studies.

Gurnià : Miss Boyd.

Agia Triada : The Italians, under Dr. Halbherr.

Samos : The Greek Archæological Society.

Rhodes : The Danes.

Delos : To be resumed by the French.

Leucas : Dr. Dörpfeld, supported by a Hollander.

SUMMARY OF ADVICE.

To sum up what has been said with regard to the opportunities for archeological research, one cannot easily, briefly, and safely compare the advantages of different sites. Between general exploration and excavation the balance might be turned by the possibility of forming a better party for one or the other sort of work. Corinth I regard as exceedingly important. Miss Boyd has done and is doing admirable work at Gurnià. The archeological exploration of the western end of Crete and of Asia Minor would be of real scientific value. The sites of Antioch on the Orontes, Laodicea, and Bœotian Thebes seem to me on the whole to be the most promising for a great excavation. Neither of the first two of these should be undertaken without the expectation of spending at least $50,000. Valuable work could be done at Thebes for less, although the sum named (to be expended in five years) would not be too great for thorough explorations there. A much smaller sum, perhaps $5,000, would suffice (so far as we can judge) for the exploration of the Minoan promontory of Megara and for trial excavations at Gythium, the port of Sparta. An exploring expedition to Asia Minor or to Crete should be dispatched, not for a single season, but for at least two. Three seasons would be still better.

CLASSICAL ARCHEOLOGY WORTHY OF SUPPORT.

Perhaps I may be allowed to add a few words with regard to the appropriateness of the work of archeological exploration in classical lands under the care and with the help of the Carnegie Institution.

(1) Classical archeology is now a science, and one in which many young scholars of our country are interested.. For the most success-

ful study of this science, a direct and intimate acquaintance with the objects of antiquity is necessary. Laboratory work is as important in the pursuit of this science as for either chemistry or physics. The best practice is secured by fresh material, which rouses the student who is engaged in research to the fullest use of his powers, and this new material is secured best by excavations, the privilege of first study and publication being always reserved for the excavator. At the present stage of the science such material is peculiarly important, and our scholars are handicapped, as compared with others, if they are not provided with it. The science of classical archeology is important in itself; it may stand alone; but as a subsidiary to the study of ancient history and philology it deserves special consideration because of its relation to general education. The ancient histories of a few years ago have little more actual value than the chemistries and geologies of the same time, and the advance in our knowledge of ancient history is due primarily and principally to the work of archeology. And from all other sources combined, ancient literature—biblical and classical—has received, I think, less light within the last third or half of a century than from archeology.

(2) Our relations to classical archeology are peculiarly close, since this is our source of information with regard to the earliest culture from which we can trace our own. America rightly feels the special obligation to learn what can be known with regard to our predecessors on the western continent, but our modern life has been influenced to no appreciable extent by the habits and deeds of the North American Indians. Our intellectual inheritance has come from the Greeks and Romans, to whom we stand in the same relations as do our English and German cousins. All the learning and devices of Egypt and the farther East have not affected us directly. The history of our alphabet may be taken as an illustration. The Phœnicians traded with all the peoples of the Mediterranean and the Black seas—very likely even with the early inhabitants of Britain—but not one of all these peoples except the Greek had the skill to adapt the Phœnician alphabet to western use. Thus also Egyptian mathematics and Chaldean astronomy were received by the West only after being digested and assimilated by the Greeks. Still less did Egyptian and Assyrian art influence directly our own sculpture and architecture. Sir Henry Maine exaggerated only slightly when he said, "Everything that is not a law of nature is in its origin Greek." Since we are the intellectual descendants of the Greeks and Romans, and claim this relationship, the study of their lives and works is as

fitting for us as for any other moderns, and we might as reasonably leave the science of astronomy or that of mathematics to the other nations of the world, sure that it would not be neglected though we did not pursue it, as to leave to the peoples of Europe the advanced study of classical archeology and philology. Our manner of life and our literature are so founded upon those of the ancients that we cannot properly understand the present without an appreciation of the past. Largely through the opportunities offered by the American School of Classical Studies at Athens and its sister school in Rome, about two hundred of the classical teachers of our country have been brought into direct relations with the antiquities of Greece and Italy. We have as yet, however, only four or five classical archeologists who in training and attainments are worthy to be classed with the European university teachers of archeology. Work in connection with an important excavation or an expedition for exploration would do much for the training of the men who are to be the leaders of the science in America and who, we hope, will advance the whole science of classical archeology.

LONDON, *September 1, 1903.*

MECHANICS OF THE HUMAN VOICE

REPORT BY E. W. SCRIPTURE.

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INTRODUCTION.

I have the honor to report on a grant of $1,600 for prosecuting researches on the voice. As stated in the original application, I had on hand a large amount of unstudied material accumulated by a new method. The method consisted essentially in registering the human voice by the latest gramophone apparatus, and then tracing off the vibrations in great enlargement by a machine which I constructed specially for the purpose. This method has the advantage over all other methods of studying the voice by preserving the original speech as a gramophone plate (or phonograph cylinder) as well as furnishing the curves.

METHODS EMPLOYED.

Two quite distinct kinds of activity were involved, namely, obtaining the curves and studying them; the organization therefore included a tracing station and a computing bureau.

At the tracing station the curves are obtained from a gramophone plate—selected or specially made to contain any desired vocal utterance—by tracing off the vibrations by a special machine. Such a machine is suspended by springs or placed on a cement floor in a rather long room (50 to 100 feet). The machine is run continuously by an electric motor. The curves are traced on long bands of smoked paper; the speech curve appears as a thin white line on a

black surface. On account of its delicacy, the machine requires daily inspection and care by a mechanic. The renewal of paper occurs once every 12 or 24 hours, according to the length of the strip of paper, a factor that depends on the length of the room. Each renewal requires work for 1½ to 2½ hours.

The strips of paper are varnished before they are removed from the machine. A narrow band containing the curve is then cut out and mounted in the form of plates on pasteboard. Each plate is covered with a sheet of celluloid and is delivered to the computing bureau.

The records referred to in these investigations had been traced off by an apparatus known as Machine A. This machine was allowed to run at Yale University till March 1, at which date it was dismounted and turned over to the university. Owing to the fact that it was the first of the machines constructed, it could be successfully used only under my constant personal supervision; owing to the many changes in its construction and to the wear of four years running (often day and night), it had become somewhat deteriorated. It was decided therefore to build two new machines on principles learned by previous experience. These are now finished. One, known as Machine D, has been at work at Yale University under care of the mechanic who built it, and has completed the tracing of a plate containing a record of Dr. S. Weir Mitchell's voice. The other, known as Machine C, was finished in Europe and is about to be set up in Berlin.

Still another machine, of a different kind, had been constructed by a grant from the Elizabeth Thompson Science Fund several years ago. It traces off curves from a French phonograph with celluloid cylinders. Forty special cylinders of French prose and verse were made for it in 1902. It ran constantly from October, 1902, to May, 1903, and furnished the first curves of French speech ever obtained. These curves can be used for solving the problems of French vowels, French melody, and French verse, just as the curves for English were used for the problem of English speech. I have with me the complete tracing of a record of *Le Roi d' Yvetot*. It awaits funds for assistance in measurement and time for study.

The work of the tracing station was carried on at Yale University until September 15.

At the computing bureau the first work consisted of measuring abscissas and ordinates of the curves on the plates and in computing results according to certain formulas and methods. The

methods of computation employed by other investigators were first tested and found to be not fully adequate; they were therefore somewhat altered and developed and the approved methods were then applied to the plates on hand.

A large amount of material had been collected during two years' work of the tracing machine. This could not be studied because the measurements and computations required so much time. It was decided to concentrate the effort at the start on this material. It was found quite impracticable to carry this out at New Haven, one reason being the difficulty of securing intelligent labor at small rates. I therefore located in Munich, hired work rooms, and obtained labor by advertisements. Any number of doctors of philosophy, university students, and retired army officers could be found at 10 to 15 cents an hour, graduates of the Realgymnasium at 7½ to 10 cents, scholars from the Realschulen at 7½ cents. The great number at disposal made it possible to select specially reliable ones. As many as fifteen persons were employed at one time. The control of these persons, the systematization of the work, and the checking of results were placed in the hands of a retired Prussian major, the Baron von Hagen, at 25 cents per hour.

The organization was developed with German minuteness. Each piece of work—for example, the analysis of a wave—was kept in a separate little book; each worker had his particular task, and the books were passed in order from one to another. Every item of work was signed by the worker; when mistakes were found the person at fault was discharged. The separate books—nearly 500 in number—were classified and inserted in larger holders, from which they could be assigned systematically for working up any problem. A card index showed the progress and the material on hand.

The analysis of a single wave of a vowel usually required a minimum of 5 hours' measurement and computation. The study of each hundred waves from various vowels thus required at least 500 hours. The measurement for the melody of a single four line stanza of verse required the time of one person for 6 hours a day for three weeks.

The work of this measuring and computing bureau was carried on at Munich until October 1, 1903.

Problems Attacked.

The great amount of material accumulated comprised records of continuous prose speech and verse in English and German; they are

the first of their kind ever obtained, other investigators having confined themselves to single vowels sung into the apparatus (Hermann, Pipping, Bevier) or to single spoken words (Pipping).

Since the entire impression that passes by the voice from speaker to hearer is contained in vibrations of the air at the mouth of the speaker, the curves of speech can be assumed to contain solutions for all the problems of vocal expression. Among these problems are the following:

1. To determine the essential characteristics whereby one part of speech is distinguished in general from another—for example, the vowel *a* from the vowel *e*; or, briefly, the acoustic nature of speech sounds, particularly of vowels. Do the vowels consist of tones from the vocal cavities that have fixed pitches (Helmholtz, Hermann) or may they be of any pitch, provided they have fixed relations of pitch among themselves (Lloyd)? Are the resonance tones of vowels related to the tone from the vocal cords as overtones to a fundamental (Helmholtz); or do they have no such relation (Hermann)? If the latter is true, what is the explanation of such an apparent physical impossibility (Rayleigh)? Are previous investigators right in looking for the essentials of a vowel in the resonance tones alone? Why is the prevailing view of the nature and action of the vocal cavities inadequate to explain the thoroughly established results?

2. To investigate the rise and fall of the voice during speech—briefly, the characteristics of the melody of speech. In song the melody is apparently of a very simple nature; in spite of its use for emotional expression, the melody of song cannot convey all the shades of feeling that are possible by the melody of speech. This melody differs for every condition of mind, for every person, for every dialect, for every language. What are its fundamental laws?

3. To investigate the characteristics of rhythm in prose speech. Both prose and verse have their elements arranged on more or less regular systems in respect to stress, duration, melody, and other elements. A complete study of these elements for verse would solve the problem of the nature of verse. The theories hitherto proposed were established on the basis of judgments by the ear alone, and they certainly all quite miss the essentials of the case. They are theories of verse as it appears in type rather than of verse as it comes from the mouth of the poet.

4. To establish the types of American vowels. There are not 10 or 12 typical vowels, as often supposed, but more nearly a hundred of them, as distinct and as indistinct from each other as the races

of men. Some approach to the truth has been reached by lexicography in respect to the long vowels, but its results are largely erroneous in respect to the short ones. Both phoneticians and lexicographers are misled by spelling, and, by a well known psychological prejudice, vowels that are physically quite different appear to the ear as the same, and *vice versa*. For example, most phoneticians distinguish two forms of the so called indefinite vowel. Sweet (Axford) has lately recorded seventeen forms. In my curves I find several hitherto unrecognized forms, physically quite distinct, for nearly every vowel.

5. To study the differences of speech among various dialects. The phonetic survey of France (Guilleron) consisted in recording by ear the pronunciation of certain typical words in each district of France. The results were published as a series of maps, each one containing in phonetic type the pronunciation of the word at each place. The ear loses most of the differences in pronunciation ; the type loses still more ; the results are vague. The proper method would be to take records by the phonograph or the gramophone (as recently done by the Vienna Academy of Sciences for certain Brazilian dialects), and then to trace the curves by my method for accurate study. I find that phonograph records could be made with indestructible metal matrices and cylinders cast and delivered at a smaller cost per word than the atlas of Guilleron. Instead of maps with phonetic type, the student would have a collection of the speaking records. Various disappearing and changing languages (American Indian, for example) might thus be collected and preserved. The records might then at any time be traced off and studied by the methods used in these researches.

6. To determine the differences due to the speaker or singer himself, and to investigate the influence of training on these differences.

7. To investigate the influence of the emotions on the voice.

These last two lines of investigation would lay the foundations for the psychology and physiology of vocal expression. It would lead to data concerning the laws of expression in vocal music and in oratory ; also possibly to the use of vocal records for determination of mental conditions in health and disease.

Of the work on these and other problems only the results for the first three will be considered in this report.

Results Obtained.

1. The Nature of Vowels.

According to Helmholtz a vowel consists of the reinforcement, by the vocal cavities, of overtones in the tone from the vocal cords, these reinforced tones lying within definite regions for each vowel. Thus, for the vowel *a* the vocal cavity is so adjusted that it reinforces a certain tone; this tone, however, as an overtone, must stand in respect to its pitch in one of the relations 1, 2, 3, etc., to the tone of the vocal cords; that is, its number of vibrations must be an even multiple of that of the cords, and cannot be, for example, $1\frac{1}{3}$ or $4\frac{7}{10}$ times as great. According to Hermann a vowel consists of the presence of a tone or tones of definite pitch, the pitch being quite independent of any tone or overtone from the vocal cords—that is, the vowel tone may have, for example, $1\frac{81}{100}$ or $5\frac{16}{17}$, as many vibrations, etc. The two theories agree in asserting the presence of tones of a definite pitch or range of pitch for a particular vowel; thus for *a* there must be a tone of one pitch in the vocal sound, for *o* a tone of another pitch, etc. The two theories disagree as to the relation of this tone to the cord tone.

Helmholtz based his theory on attempts to manufacture vowels by reed pipes and by forks, with results that were not satisfactory. Hermann based his on curves of vowels sung by his own voice into a phonograph and then traced off. The material in either case was extremely limited. The results now studied under this grant comprised several hundred vowels by different speakers; being curves of really *spoken* vowels, they required new methods of treatment and gave results quite different from the rather artificial vowels of song.

It is to be understood that this work consisted of collecting specimens of speech of clearly evident excellence on gramophone plates, of tracing off the vibrations on paper in great enlargement, and of analyzing the waves of the curves by means of microscope measurement and mathematical formulas. Each single vibration of a vowel, for example, could be analyzed to show the set of tones from the cords and the vocal cavities. Each result stands as a fixed datum with which any theory has to reckon.

The results soon showed that neither the theory of Helmholtz nor that of Hermann could be applied. No explanation or new theory could be found until a suggestion was received from another investigation, as follows:

The validity of a vowel theory can be tested by constructing an apparatus to produce vowels on its principles. The Helmholtz theory was tested by Helmholtz himself in his vowel apparatus. This consisted of a series of tuning forks with pitch frequencies in the relations of 1: 2: 3, etc. By adjusting the resonators different overtones could be reinforced. Helmholtz states that he could obtain only a good *o* and *u*. The same results can be obtained almost as well by simpler apparatus and in a way to refute the theory. *Any* well made tuning fork of any moderately low pitch will produce a good *u*. Empty bottles often give good *u* vowels through several octaves of pitch. These facts made it unnecessary to construct another apparatus on the Helmholtz theory. I therefore began on the Hermann theory, which at the start I believed to be correct. According to this theory the vocal cords emit a series of puffs of air, each of which acts like a blow on the air of the vocal cavities and arouses the tones of the cavities. A blow of the hand on the open mouth will thus arouse the cavity tones. If we conceive the blow to be repeated often enough —*e. g.*, 100 or 200 times a second—we should hear a vowel. To produce these puffs of air I first used a siren comprising a disc with holes rotated before a blast of air. I found that a resonator with hard walls (brass, wood, etc.) would respond only when the puffs came at a definite pitch, whereas resonators with soft walls (cotton soaked in water, gelatine, etc.) would respond to any pitch. I could thus produce good examples of *a*, *o*, and *u* with a siren tone of any pitch. I then tried a *vox humana* organ pipe with the same results. These vowels were produced at *any* pitch of tone, as required by the Hermann theory. I utterly failed, however, to produce *e*, *æ*, or *i*.

Certain puzzling phenomena in the speech curves seemed to indicate that the action of the vocal cords differed for different vowels. Such an unusual conclusion, though difficult to accept, might be justified by considering (1) that the innervation of different parts of the vocal cords may differ for different vowels, whereby the distribution of the load, and consequently the mode of vibration, may differ (it should be noted that the vocal cords, or, more properly, the vocal bands, do not vibrate merely along the thickened edges, but through the entire muscular mass); and (2) that the vocal cords as soft bodies may be influenced in the character of their vibrations by the size of the resonance cavities. The experiments in manufacturing vowels were now renewed with tones from rubber membranes.

It was found that a combination of soft walled resonators with a yielding source of tone was adequate to produce all the vowels.

These experiments led to two conclusions: first, that one element in the nature of a vowel is a modification of the action of the vocal cords themselves, and, second, that it is possible to manufacture a machine that will sing the vowels.

With the first conclusion at hand the study of the curves was renewed from a different point of view and new methods of analysis were devised. The curves were found to be explainable on this theory. We thus conclude that in speech the vocal cords do not execute their vibrations independently of the adjustment of the cavities above them, but that the form of vibration differs for different vowels. Thus, when the vowels *a*, *e*, *i*, etc., are spoken at the same pitch, the vocal cords vibrate so as to emit puffs of air of different force and consequently of different timbre. One element in the nature of a vowel therefore consists of a modification of the vibration of the vocal cords—a result that, as far as I am aware, has never been suggested before. The consequences for phonetics, laryngology, and vocal culture have not been deduced.

The element of soft resonators must be considered at this point. The vocal cavities have hitherto been treated like resonators of brass or glass and conclusions have been deduced on this basis. Lord Rayleigh, for example, states that for very high tones the Hermann view of the independence of the cavity tone from the cord tone may be correct, but that for tones within an octave or two of the cord tone the Helmholtz overtone theory is certainly valid—a statement that would hold good for cavities made of brass or other hard material, but that is contradicted by every vowel curve obtained. The explanation is simple: the flesh forming much of the walls of the vocal cavities is soft, and nearly all the rest is covered by a moist membrane. The phenomena of resonance are quite different. As stated before, a brass resonator responds only to a tone or tones of definite pitch, whereas a resonator with walls of water responds to *any* tone. A series of artificial heads made of metal with the proper adjustments of the cavities could not be made to speak the vowels, but a series made of gelatine or similar material should do so. A mathematical treatment of the laws of resonance for soft cavities has not yet been developed, although it would be of value.

The theory based on the experiments in making vowels and the conclusive evidence for the speech curves shows how we must proceed in order to make an efficient vowel machine. The materials I

have used—gelatine, water, etc.—have to be renewed every few days. If they can be replaced by durable materials a vocal organ can be built that will sing vowels at any pitch, giving true human tones instead of the bleating tones of the present *vox humana* register. An apparatus of this kind is now being constructed on funds from another source ; if successful it can be added to the regular church organs and used to sing the vowels during chants.

On the basis of the principles just stated, namely, modification of the cord vibrations by the adjustment of the cavities and softness of the cavity walls, work on the curves has led to the following further conclusions.

The resonance tones of vowels have definite regions of pitch which have definite relations to one another. For the effect on the ear changes in the region of pitch may be compensated by changes in the relation and by the introduction of new tones. Both Helmholtz and Hermann are therefore correct in assuming in general definite regions of tone for a particular vowel, but the tones can vary greatly if only the necessary compensations are made to deceive the ear.

It is to be noted that in their essentials the apparent contradiction between the theories of Helmholtz and Hermann disappears. We can, like Hermann, assert that the cavity tones are the independent variables ; but when we add that the cavities modify the mode of vibration of the vocal cords so as to bring out certain overtones from the cord tone, we reach a result essentially in agreement with the theory of Helmholtz.

A great difficulty in studying the speech curves lay in assigning any physical meaning to a single wave of the curve. The curves did not permit any interpretations according to the usual views of resonance of overtones ; otherwise mathematical analysis of Fourier would have furnished immediate results, and harmonic analyzers, like that of Michelson, would have made it possible to spare a great amount of work. A mathematical treatment of vibrations on new principles was out of my power, but a physical synthesis was attempted. A machine was constructed to register the curves of two superimposed vibrating springs of different dampings acted upon by magnetic impulses of different degrees of suddenness. If it can be made to furnish curves like those obtained from speech, we shall have the following results : 1. The principles of vibration on which it acts can then be assumed to be the same as those of the voice. 2. An atlas of curves can be prepared in such a way that when a given speech curve is found to be like a specimen in the

atlas, its components will be given at once by the known components of the apparatus curve, thus avoiding a long and expensive analysis. 3. Curves of pure speech sounds can be directly engraved on gramophone plates without any intervention of the voice itself. This apparatus, made in Munich, after a former rough model, is nearly completed.

In studying the curves another phenomenon was observed. A spoken vowel changes its character from beginning to end; in the hundreds of vowels inspected no two waves of a vowel were ever exactly alike. Neighboring waves were similar and the change occurred more or less gradually, according to the nature of the vowel. Except in long vowels the ear cannot detect even the existence of such a change, and all phoneticians have considered the short vowels as constant things. The ear gets only an impression of the general effect and what the vowel really sounded like at beginning, middle, and end remains unknown. I find that the ear classifies as the same short vowels a number of types having in reality no acoustic resemblances. I also find that a suggestion from spelling or from another word will cause the ear to hear as different several vowels that are acoustically the same, and as the same several vowels that are acoustically different. The phonetic spellings in the dictionaries are certainly to a considerable degree erroneous in respect to the short vowels. The study of short vowels should be extended to include many hundreds of cases, and a machine should be constructed that will make it possible for the ear to hear each stage of the vowel separately. In respect to the long vowels a somewhat similar condition was found; for example, the usual statement that long vowels, such as *ee* in "see," are diphthongized does not always, or even generally, hold good in ordinary American conversation.

To elucidate the curves of vibration from the vocal cavities it was necessary to make some study of the cavities themselves—for example, as formed by the position of the tongue. One method of studying the position of the tongue is that of employing an artificial palate with a chalked surface; the chalk is removed where the tongue touches. This method was devised in 1887 by the American physician Kingsley, who registered about a dozen contacts. For French the study has been systematically carried out by many hundreds of registrations by Rousselot (Paris) and his pupils; for German a few results have been obtained; for English nothing has been done. I have accumulated and prepared for publication many hundreds of registrations of the American sounds in their varieties, using not

only the former palate of hard rubber, but also a specially thin one of aluminum. I have also lately devised a palate of paper soaked in cobalt chloride, which registers automatically the regions of contact by changing the color of its surface.

2. The Melody of Speech.

Under melody we understand the rise and fall in pitch of the tone from the vocal cords. The pitch of the cord tone at any instant can be obtained from the speech curve by measuring the length of the group of waves corresponding to one vibration of the cords. An extensive study of English melody is of the highest importance for oratory, for the history of the language, for the psychology of the emotions, and, according to the recent discovery of Professor Sievers (Leipzig), as a means of textual criticism. Only three experimental studies of English melody have yet been made, all in my previous investigations. In the present investigation part of a speech by Senator Depew was studied ; it furnished data concerning American melody in a speech without oratorical exaggeration. A record by another speaker furnished data for American speech with satirical expression. The melody of Rip Van Winkle's toast, by Joseph Jefferson (previously published), was recalculated by more accurate methods ; it furnished data for emotional expression. In this way the characteristics of American melody for oratorical speech were obtained. When compared with the previous studies of the melody of American sentences and of the Lord's Prayer, the differences from conversational and religious melody became apparent. The work on melody should be extended to more persons, to different subjects, and to different languages. Problems of the following kind would be answered :

(*a*) Has each piece of prose or verse a charcteristic melody of its own that appears in spite of the individual differences among the speakers ? If so, is it possible on this principle to pick out collaborations and insertions from a text ? Professor Sievers asserts this to be true, and is just issuing an edition of the Hebrew bible with text criticism on this basis. His judgments are entirely by the ear as he reads the passages himself. The fundamental principles, however, should be established by experimental records. I discussed the matter last spring with Professors Wundt and Sievers in Leipzig, and the Psychological Laboratory there has, as a result, undertaken of its own accord some researches on German melody. At the

University of Munich the interest in this work led to loan of apparatus from the Psychological Laboratory and to active assistance by Professor Lipps.

(*b*) How does the melody of any given piece or of conversation vary with the emotions, with the speaker, with the dialect, etc.? Have the differences in melody anything to do with differences in character among different persons and among different communities? For example, does the rising inflection at the end of each sentence indicate as a permanent trait for all Saxony the dubitative feeling that it indicates when used by a person in any other part of Germany?

Quite a number of records of melody have been completely studied, but the work should be considerably extended before conclusions are finally drawn.

3. The Rhythm of Speech.

The same record that furnishes the data concerning the nature of the sounds (see 1, above) and the melody (see 2, above) also furnishes measurements of their length, or duration, and approximately of their stress. These are the data for deductions concerning rhythm or meter, and consequently of the solution of such problems as:

(1) The nature of English verse. Is it mainly quantitative (*i. e.*, distinguished by long and short syllables), or melodic (*i. e.*, by rise and fall of the voice), or emphatic (*i. e.*, by greater or less stress)? These are the points under discussion at present. My results lead to the conclusion that such questions are entirely aside from the essentials of the case. Verse, I consider, is a form of mental expression with periodic recurrences of greater and less mental effort. The problem is, then, to find how this greater effort expresses itself. I find that it may occur not only by greater stress or by longer duration, but also by a change in pitch, by decreased stress, by shorter duration, by difficulty of enunciation, by pauses, by contrast of thought, by emotional content, or by any other element that is appropriate to increased expression. This psychological theory of the nature of verse renders it possible to gather the various forms into one system explainable on common principles. The usual division of English verse into feet and syllables has no poetical meaning; no poet writes that way.

For carrying this investigation into detail I have had two gramophone records of verse made by an American poet. The tracing off of one of these required over two months' running of the machine;

it is now finished. The work should be promptly extended to records made by the greatest living poets.

(2) The nature of emotional expression in oratory. Prose has its rhythm also. The most melodious orations have rhythms often resembling those of verse. The changes in these rhythms, the introduction of rhythmic discords, etc., are unconsciously used by orators to arouse the emotions. The three speeches studied furnished a quantity of data.

Continuance of the Work.

1. Publication of the results already obtained can be undertaken, but it is not recommended at present. The records studied are mounted as 40 plates for reproduction. Of these 18 are already in blocks, leaving 22 still to be made. The only satisfactory method thus far found for reproducing the plates is that of copper etching, as used by C. P. Wright, West Fourteenth street, New York city, at a cost of $15 to $20 per plate. The preparation of the text of 400 octavo pages to go with the plates will require about two months more of labor. Not one fifth, however, of the information on the plates has yet been extracted, and the work should, in my opinion, be completed, before publication in many of its details, by further study of the plates already on hand and by work on new plates already furnished from the tracing station.

2. Provision should be made for the study of results already on hand. The study of the plates known as the "Yale record" (specially made for me), the "Woman record," the "Mitchell record number 3" (specially made), and the "A K A record" (specially made); also of the "Yvetot record" (specially made, French verse) should be at once continued under my personal care. For this I need to employ the labor of 3 persons for 6 hours a day for 150 days. At 20 cents an hour this requires $540. The incidental expenses connected with cutting, pasting, photographing, and measuring the records can be estimated at $150. The total outlay proposed would therefore be $690.

3. Provisions should be made for continuing the tracing of speech curves.

Machine D (gramophone records) should be kept running for the next six months, tracing off a set of plates (American vowels, American verse, etc.). This machine gives larger and more detailed curves than those obtained by the other machines. As the

records are specially made, the material is of such appropriateness that it could be profitably published at once as an atlas with instructions for study. This would place a great amount of new data in the hands of psychologists and linguists for investigation and publication; or publication may be reserved until arrangements are made for studying the results.

Machine B (French curves) should also be kept running for six months. During this time the machine can trace a record of French prose, a record of French verse, and a record of French words, each one of which, with the exception of unstudied records already in my possession, will be the only accurate curves of French speech ever obtained, and will furnish, for the first time, accurate data concerning the French vowels, French melody, French rhythm, and French meter. The curves could profitably be published at once as an atlas of plates with instructions concerning methods of study. If publication were postponed, Professor Grandgent, of Harvard, would presumably assign topics for doctor theses and deliver results for publication with the curves. As these curves would contain material for 8 or 10 theses, and as the interest is widely spread, I recommend the prompt publication of the plates alone.

These machines can be handled only by myself and the mechanic who has had four years' experience in making and running them. This mechanic (C. S. Smith, 119 Davenport avenue, New Haven, Conn.) is willing to continue the work at $18 a week, or $450 for the next six months. The incidental expenses—paper, paste, electricity, repairs, etc.—can be estimated at $75, making a total of $525. These two machines will run in his charge in America. The results will be delivered to me as my property, subject to the wishes of the Carnegie Institution in respect to publication.

4. Further new gramophone records should be specially made for these investigations. My experience has led me to formulate the following principles:

(*a*) The persons selected for the records should be of such prominence that their biographies are and will be obtainable from cyclopedias, histories, and biographical dictionaries by any one studying the results. I often find it difficult to obtain the requisite data directly from persons of no distinction. All my efforts have been fruitless to obtain, directly or indirectly, any information from the still living speaker of the first record I studied. In any case, persons elsewhere may study such speech curves from other points of view and may wish different biographical data from the ones I have

collected. Still more important is the consideration that a decade or more hence entirely different and unforeseen problems will be considered of the most importance, and, although the data can be found in the curves, the personal data will be lacking.

(*b*) The metal matrices for all records should be preserved, in duplicate or triplicate, in fireproof buildings, and copies of the record should be obtainable by any one at reasonable cost. One of my most important studies has been of a private record by Joseph Jefferson. I possess only two copies of the record, one of them already much worn. No more copies can be obtained, and the matrix cannot be found. No other record of Jefferson's voice exists. Of another record all my copies but one have been worn out, and the matrix is known to have been destroyed.

These considerations suggest the following plans: For all future records specially made for me the American and German gramophone companies have offered to furnish—in strictest privacy and free of charge—duplicate or triplicate metal copies (one in indestructible material) of the matrix, provided the records shall be of such historical and scientific value as to justify the cost (a minimum of $100 for each record) and provided proper places of deposit shall be found. The company will consider these records as the property of the person speaking, of the place of deposit, or of myself, according as I may adjust the matter, and binds itself to make no use whatever of these records for its own purposes. I have already arranged with the National Museum, with the Library of Congress, and with Harvard University for depositing such matrices and records. In compiling a list of persons whose voices would, for one reason or another, presumably be of historical importance in the future, and thus worthy of the expense of registration and preservation, I have had the advice of President Gilman, President Eliot, Mr. John Hay, Professor Asaph Hall, Mr. Henry C. Lea, Mr. Joseph Jefferson, Mr. John La Farge, and others. These records will form the nucleus of a phonetic archive of the voices of distinguished or interesting persons. A similar archive is to be founded in Berlin, and I am now arranging for an interview with his Majesty the German Emperor. Copies of such European matrices and records as may be of interest to Americans will be delivered to me for deposit and study. In America I propose to collect the voices of prominent statesmen, business men, scientists, writers, divines, orators, singers, actors, and others. In Europe I shall begin with the voices of rulers. In the collection and investigation of such specially made

records great weight should be laid on those of poets and orators. In the first place, the printed verse or speech can convey only a part of the intention of the author; a good gramophone record by Patrick Henry or Longfellow would give something that is now forever lost. In the second place, from accurate speech curves traced from such plates we can obtain for the first time reliable data concerning the essential factors of oratorical speech and of verse.

The plans on both sides of the Atlantic are complete. I am ready to carry them out privately or under the direction of the Carnegie Institution, according as the Trustees may direct. In the former case the matrices and records will remain my personal property. There will be no expense except for traveling.

5. Provision should be made for the construction of the following apparatus:

One piece, suggested by Dr. Billings, is designed to turn the records of the speech curves back into sound. The difficulties with the design have been great, but have now been overcome. The success of a speech record depends on allowing time for the point of the reproducer to follow the fluctuations of the groove. The larger the disc or cylinder the better the result. The speech waves are thus very long and flat, and often cannot be distinguished by the eye from a straight line. In my tracing machine the amplitude of the fluctuation is greatly increased, while the length of a wave remains the same, or is even shortened. Thus the fluctuations become visible. Such waves cannot be used for producing sounds directly. They must be greatly drawn out in length and reduced in amplitude. For this purpose the speech curve is in the new apparatus to be cut on a wooden cylinder and reproduced as a depression in wax on a phonograph cylinder. The former cylinder is rotated at a very slow speed in comparison with that of the latter, while the amplitude is appropriately decreased. The minor technical difficulties are, of course, numerous, but the machine can presumably be finished before next spring.

The value of such a machine lies not only in the fact that it can be used to test the accuracy of a tracing by turning it back into sound (it can be more readily tested in other ways), but particuarly in the fact that it opens up an entirely new method of studying the voice. A vowel consists of a series of vibrations that are not of constant character. The form of the curve always (in my results for American English) changes from beginning to end, wave by wave. The sound must therefore be different at each instant.

Except in very long vowels, the ear cannot hear any change, but gets a general impression. This is necessarily so crude that trained phoneticians will assume utterly different vowels in such a word as "not," even when spoken on the same occasion by the same speaker. The changes within the vowel lie, of course, beyond the grasp of the ear. This new machine will furnish the means of an *acoustic analysis* of the vowels in the following manner: The first wave of the vowel, as it appears in the tracing, is engraved on the wooden cylinder in a continuous repetition. The reproduction on the wax cylinder then gives continuously the sound for the first wave of the vowel. This is done for each wave in succession. In this way the acoustic vowel elements are determined throughout a vowel. The cost of this apparatus can be placed at $300.

A special measuring machine should be constructed. At present the curves are enlarged by photography and measured directly in tenths of a millimeter. To avoid the enlargement, an apparatus was designed in which a magnifying glass can be moved by millimeter screws in two directions above the curve without touching it, the readings being in hundredths of a millimeter. The construction was not begun, as the estimated cost (mainly for the fine screws) was $200. It should be made at once, however, as the photography is a heavy running expense, which can be avoided once for all by the apparatus.

6. A mathematical treatment of the phenomena in resonators with soft walls and of the vibrations of soft bodies should be sought. In the works of Helmholtz and Rayleigh these problems are quite overlooked, and yet the human voice—whose vibrations are produced in this way—is the most important of musical instruments. Lord Rayleigh, in the second edition of his Theory of Sound, has shown great interest in the study of vowel sounds. I suggest that he be requested by the Carnegie Institution directly to treat the subject. I would take great pleasure in laying before him any desired curves of speech or results and in carrying out any experiments, measurements, or computations he may wish. If he is not willing to do the work I shall be pleased to cooperate with any one whom the Institution may suggest. Presumably, no cost will be involved unless Lord Rayleigh desires a personal interview in England. In this case traveling expenses will have to be covered.

FUNDAMENTAL PROBLEMS OF GEOLOGY

Report by T. C. Chamberlin.

I have the honor to submit the following report of progress on the work done under Grant No. 31, made for the promotion of investigation on certain fundamental questions in geology. The period covers nine months, from January 1 to October 1, 1903.

Immediately on the announcement of the grant an arrangement was made with the University of Chicago by which I was relieved of all educational work except that directly connected with investigation, and even this class of university service was limited to six months of the year. Arrangements were also effected for relieving me, so far as practicable, from executive work. A research assistant was provided, a feature of the understanding being that the University would support at least as much investigation in geological lines as it had done before. The University has continued to pay one half of my previous salary, and the other half has been paid from the grant.

Plans for collaboration on different phases of the complex subjects of investigation, of different extents and degrees, were soon arranged with Dr. F. R. Moulton, of the University of Chicago, on questions relative to the origin of the earth and related subjects, especially those involving celestial mechanics; with Professor L. M. Hoskins, of the Engineering Department of Stanford University, on questions relating to earth stresses and cognate subjects; with Professor C. S. Slichter, of the Mathematical Department of the University of Wisconsin, on questions connected with the rotational effects of tidal action; with Mr. A. C. Lunn, of the Mathematical Department of the University of Chicago, on questions relative to the generation and distribution of internal heat by compression and related subjects; and with Professor Julius Stieglitz, of the Department of Chemistry of the University of Chicago, relative to the bearing of certain ancient evaporation products on the former state of the atmosphere. Mr. W. H. Emmons, of the Department of Geology of the University of Chicago, has served as research assistant under appointment by the University of Chicago.

In pursuance of the plan of collaboration with Dr. Moulton, it was thought best at the outset to examine critically everything of any

moment that had been written on the origin of the solar system. This had not advanced far before the conviction arose that the preparation of an analytical review of the whole literature of the subject would save other investigators the very considerable labor of repeating the task undertaken by Dr. Moulton and would in other respects be a valuable contribution to investigation. In pursuance of this the literature, which is quite voluminous, has all been read with extensive annotations, and the preparation of the review is now in progress and will be submitted as soon as finished. Subsidiary to this, Dr. Moulton has aided in the informal discussion of other subjects under investigation.

As the origin and distribution of the earth's internal heat is fundamental to many of the most important problems of geology, a large part of our joint studies has been given to the continuation of previous studies bearing on this subject; but, as all fundamental questions react the one upon the other, we have endeavored by the scheme of collaboration adopted to advance coordinately several lines that seemed most promising of mutual helpfulness.

An analysis of the work heretofore done on the origin and distribution of internal heat and of its sequences reveals the fact that inquiry has largely been guided by the older and the least probable of the three hypotheses of the origin and initial distribution of heat that are now recognized. Under the long dominant view that the earth descended from a gaseous spheroid through the molten state to its present condition, there naturally arose the conception that during the liquid state convections stirred the molten mass from center to circumference and measurably equalized the temperature, so that the whole was cooled down equably until it approached the temperature of solidification and became too viscous for further effective convection. As the effects of internal pressure on the temperature of solidification were unknown in the earlier days, and largely remain so still, a *uniform* internal temperature, not far from the melting temperature at the surface, was postulated. The deductions of Lord Kelvin, which have exercised great influence on geological opinion for the past half century, are based on the assumption of a uniform initial temperature of 7,000° F. throughout the interior, and similar assumptions of practical uniformity, with unimportant numerical variations, have been made by other able investigators. Computations on this basis, with reasonable assumptions relative to the subsidiary factors, lead to the conclusion that effective cooling could scarcely exceed a depth of 200 miles in a period of 100,000,000

years, and not more than 350 or 400 miles in the enormous period of 600,000,000 years. The inference inevitably follows that all the movements and deformations of the crust of the earth, due to cooling, are confined to very shallow depths relative to the radius of the earth, and that the greater interior mass of the geoid has not appreciably participated in thermal effects. This conclusion, if true, is radical. Assumed to be true, it has profoundly influenced geological inquiries and interpretations.

With the growth of evidence that the temperature of solidification rises with pressure, there has grown the hypothesis that the earth solidified first at the center, because of the high pressure there resident, and later congealed at higher and higher horizons in succession until the solidification reached the surface. This view has come to replace the older view to a large extent, in competent opinion, but the necessary corollary that the initial temperature of the solidified globe would be high at the center and grade thence to the surface, though recognized, has not replaced in an equal degree, in dynamical studies, the older view of an essentially uniform thermal distribution. As a result, we have no system of dynamical doctrine worked out consistently on this later hypothesis.

In attempting some years ago to apply the kinetic theory of gases to atmospheric problems, I became impressed with the weakness of the gaseous hypothesis of the earliest stages of the earth's evolution, and subsequent studies of the relations of mass and momenta, with the essential aid of Dr. Moulton, led to still graver doubts as to the tenability of the Laplacian hypothesis, on which both of the preceding conceptions of the origin and distribution of internal heat are based. This led to studies upon alternative hypotheses. Among these is the conception that the earth, instead of descending from a gaseous spheroid, may have been built up by the gradual ingathering of its material from a scattered meteoroidal or planetesimal condition. If the infall were sufficiently rapid a molten, and even a gaseous, condition would result, but if the infall were slow the surface heat arising from impact might be radiated away practically as fast as generated, and the accretion might proceed with comparatively cool surface temperatures. The source of the obviously high internal temperatures of the earth then arose as a question crucial to the hypothesis. The suggestion that it might be due to gravitative compression arising from the earth's increasing mass was submitted to preliminary computation with favorable results. This therefore appeared to give a third working hypothesis of the origin

and distribution of internal heat. In this case, manifestly, the greatest heat would arise where there was the most compression, and the distribution of internal temperature would have a genetic relation to the distribution of internal pressures. As the second hypothesis also makes pressure a genetic factor in determining the temperature of solidification, these two hypotheses have much in common, so far as thermal distribution is concerned. In their geological bearings they differ somewhat radically from the heretofore more prevalent hypothesis of uniform initial temperature; for, if the heat gradient rises continuously—though not uniformly—to the center, there must be an outward flow of heat at all points, and hence the whole body of the earth must participate in the thermal changes, and in the effects of these on earth movement and deformation.

In these preliminary studies it was observed that the nature of the pressure curve derived from the Laplacian law of density, or from any other probable law, was such as to suggest that there would ensue an internal redistribution of heat of such a nature that the outer portions, neglecting the surface shell, might experience a rise of temperature independently of any surface loss of heat.

These considerations, briefly outlined here to render more intelligible our working scheme, are so fundamental that it has seemed worth while to spend some time in definitely developing the three hypotheses into such specific forms as would secure from them their best working stimulus and their greatest suggestiveness. This I have attempted to do.

The origin and distribution of internal heat being thus fundamental and critical, Mr. A. C. Lunn has undertaken a mathematical inquiry into the thermal effects of compression under the accretion hypothesis. As the data relative to compressibility, conductivity, specific heat, and other factors involved cover only the small range of conditions available in experimentation, and as even the possibilities of experimental determination have been as yet by no means exhausted, either in range or in accuracy, the method of multiple hypotheses has been used to cover the probable range of variation that may arise under interior conditions. The work is also being formulated so that further extensions of application may be easily made if required. While it is to be regretted that laboratory data are not better, there is compensation in the fact that such an inquiry as is now in hand may show in what lines the tedious and expensive work of experimental determination may be directed with the greatest prospect of fruitful results and what lines may be avoided as

likely to prove fruitless. A function of preliminary mathematical inquiry is to discover productive fields, as well as desert tracts, before serious and expensive cultivation is attempted.

More specifically, Mr. Lunn has had under consideration the following problems:

(*a*) The determination of the original distribution of temperature in a solid earth where heat energy is supposed to have arisen from gravitative compression, on various assumptions as to internal density and specific heat. The assumptions most closely related to the Legendre-Laplace law of internal density have been given precedence. The determinations, however, are given such forms that a short computation is sufficient to reduce to numbers the results of any assumed law likely to be entertained at any future time.

(*b*) The determination of the total heat energy due to a merely gravitative contraction of the earth mass, the assumptions in this case corresponding to those under (*a*). The development involves the proof of the consistency of the points of view assumed in (*a*) and (*b*) relative, the one to the distribution of heat, the other to its total amount. Comparison has also been instituted between the chief bodies of the solar system, in these particulars, for the sake of the collateral light they may throw on the earth problem. The immediate inquiry does not contemplate more than first approximations, but it is hoped that it will show the lines along which further and more involved researches may best be attempted, by excluding barren ground and narrowing the range of hopeful inquiry.

(*c*) Postulating the previous determinations of the initial distribution of temperature, a theoretical determination of the history of the cooling of the earth has been attempted, assuming various laws as to conductivity. This has been found to offer very serious difficulties, both in the analysis and construction of formulas, and in the complexity of the computations. Enough has been done, however, to show the probable validity of the conception, derived from preliminary studies, that the initial stages of redistribution of heat involved an actual increase of temperature in regions just below the outer shell, and that this extended over some length of time from the beginning of the process.

(*d*) Some attention has been given to such collateral problems in elasticity as were likely to throw light on the assumptions made in the previous computations, especially those calculated to supplement the meager experimental data. Mr. Lunn has also cooperated in the informal discussion of other themes embraced in our joint studies.

Since experimental data relative to the compressibility of rock must, at the best, always be limited to a rather narrow range, it is important to make such use as is possible of the geological evidence relative to the actual compression that has taken place. For the statistical data bearing on this we must look to geological measurements of the extent of the deformations that have been suffered by the earth. In the interpretation of these the assignable sources of the compressive agencies and their modes of action are important. As one step in this direction, an attempt has been made to test the validity of the suggestion, growing out of the researches of G. H. Darwin, that a change in the rate of rotation of the earth, due to tidal action, has reduced the volume of the earth by compression arising from increased gravity, and has also reduced the surface area by change of form, in addition to the reduction due to lessened volume. As Professor Slichter had previously made computations bearing on this subject, his cooperation was sought, and generously given. As a convenient approximation, a homogeneous earth had been assumed in the previous computations. For a closer approximation, he recomputed the reduction in surface area on the basis of an earth whose interior density changed according to the Laplacian law. For a broader application, the computations were made to include a series of fourteen rotation periods, ranging from 3.82 hours to 23.934 hours. The following factors were computed for each period: polar radius, equatorial radius, ellipticity, equatorial attraction, polar attraction, centripetal acceleration at the equator, latitude of mean radius, equatorial contraction (in percentage and in miles), and meridional contraction (in percentage and in miles).

It will be seen that these data cover a multitude of special cases and may be used in almost any case liable to arise. The mode of application and the general tenor of the results may be illustrated as follows: With a change of rotation from 3.82 hours to the present rate, the equatorial belt must shorten 1,131 miles, while the meridional circles must lengthen 495 miles. If the crust shortening involved in the formation of the Alps be taken at 75 miles, regarded as a very ample if not excessive estimate, the 1,131 miles of equatorial shortening would be sufficient to form 15 mountain ranges of Alpine magnitude. These should run across the equatorial belt and die away at the latitude of mean radius, 32° 22′. The contemporaneous high latitude tension would be sufficient to cause the crust to gap or to stretch in the polar arcs more than 200 miles. The coordinate geological inquiry shows no such remarkable distri-

bution of thrust and tension. Obviously the rocks of earliest formations should show the differential effects of change of rotation most notably, but the Archean formations of high latitudes, where tension should prevail, are crumpled and crushed much the same as in low latitudes, where thrust should prevail. Mountains of all ages are about as abundant north of 33° latitude as in the equatorial belt. Nor are the equatorial mountains notably transverse to the equator, or limited in extent to the demands of the hypothesis. Furthermore, if there had been appreciable change in the form of the earth to accommodate itself to a slower rotation, the water on the surface, being the most mobile element, should have gathered toward the poles, and the less mobile solid earth should have protruded about the equator, but the distribution of land and water, present and past, gives no clear evidence of this. The equatorial belt contains a less percentage of land than the area north of it, and more than that south of it. It varies but slightly from the average for the whole globe.

Geological evidence being thus out of harmony with deductions from the researches of G. H. Darwin on the earth-moon evolution under tidal control, inquiry as to the source of the discrepancy was naturally invited. The influence of the ocean tides is probably ineffectual because the varied positions of the derived tides are such as to nearly neutralize their own effects mutually. This conclusion has also been announced recently by Poincaré.* The essential question, therefore, resides in an assumed body tide, and this brings into sharp emphasis the question whether the earth is not either too rigid to give an effective body tide, or so resilient, owing to its high elasticity, that the tides do not have the right position to be effective in retardation; for retardation of rotation is as much dependent on the carrying forward of the tidal protuberance by the earth's rotation as on the amount of the protuberance.

We have endeavored, therefore, to advance the study to include the distribution of rigidity in the earth, as well as its amount, and to recognize the effects of elasticity on the response and the resilience of the spheroid to the tidal stresses. Tentative laws of distribution of the internal rigidity have been formulated, two of which may serve a temporary purpose until better grounds can be developed. At the present stage of inquiry, it would seem that geo-

* L'influence des marées océanienes sur la durée du jour est donc tout à fait minime et n'est nullement comparable à l'effet des marées dues à la viscosité et à l'elasticité de la partie solide du globe, effet sur lequel M. Darwin à insisté dans une serie de mémoires du plus haut intérêt. (Bulletin Astronomique, tome XX (June, 1903), p. 223.)

logical evidence favors extremely high rigidity and elasticity, in contradistinction to the viscous, and even fluid, implications so long urged on the basis of certain geological phenomena.

Professor Hoskins has recently computed the effective rigidity of the earth from the periodic variation of latitude, using methods somewhat different from those of Newcomb, Hough, and Woodward, and reaching results in such a form as to be directly applicable to the problem of tidal retardation of the earth's rotation. These results give the basis for a new and promising line of approach to the problem of tidal retardation, for they permit the substitution of a definite rigidity, determined on independent grounds, for the assumed viscosity on which Darwin's classic investigation was founded. Without going into details, it will suffice to indicate the general bearing of the investigation to say that the recomputed rate of retardation is only about one fourth as great as that found by Darwin. In reaching this result the *position* of the tide was assumed to be that most favorable to retardation. The determination of the actual position that the tide would assume with the given rigidity and elasticity has not yet been attempted, and the amount by which it will modify the above result is unknown. If it modifies it at all it will be in the nature of a reduction and will further tend to bring the results of computation more into harmony with the geological evidences. The same line of inquiry promises other results of value relative to the state of the earth's interior.

Previous studies had led to the conviction that the stress accumulating competency of the earth's body affords a promising line of approach to the physical state of its interior, and Professor Hoskins has cooperated in certain preliminary steps intended to test the validity of this conviction and to develop the problem. The general line of reasoning may be briefly indicated. Innumerable gentle warpings have affected nearly every portion of the surface of the globe at nearly all stages of its history. This implies the perpetual activity of minor forces of deformation and a concurrent yielding of the outer part of the earth to these forces. At the same time, when the master phenomena of movement and deformation are considered, there appear to have been long periods of relative quiescence, followed by epochs of profound deformation. This general view, long held by leading geologists, is being greatly strengthened by the working out of the great baselevels, which add evidence of the most cogent kind relative to the quiescent stages, while the stratigraphic evidence of the periodicity of the great deformations is regarded as

ample. As the known fundamental agencies, such as the loss or the redistribution of heat, work constantly rather than periodically, it is inferred that the stresses arising from them are sustained by the strength of the body of the earth until they have accumulated to an intensity sufficient to compel yielding and consequent deformation. The magnitude of the deformations being measurably determinable from geological data, the problem is to determine what states of the interior matter are required to accumulate such stresses, and how large portions of the mass, in these requisite states, must have been involved to meet the requirements of the case. The preliminary tests seem to indicate that at least a large portion of the whole globe must have been involved, and that the effective resistance of this must have been of a high order. These results tally well thus far with those derived from studies on the rigidity of the earth on independent lines. Professor Hoskins's other engagements have not permitted him to seriously attack the more difficult phases of this promising line of inquiry.

The logical connection between atmospheric studies and the foregoing problems may not be evident, but it is quite real. My own special interest in them sprang from climatic problems which led back through the history of the atmosphere to primitive states and fundamental conditions. A special study of a gypsum deposit of Iowa, conducted in part under my supervision, by a fellow of the university, Mr. F. A. Wilder, seemed to the suggestive mind of Dr. Stieglitz to afford a means of testing the atmospheric conditions relative to the critical element, carbon dioxide, at the time of the precipitation of the gypsum, which Dr. Wilder interprets as Permian. At my request, Dr. Stieglitz has undertaken the investigation. It is not yet complete, but its nature may be indicated. The significant feature of the gypsum deposit is its remarkable freedom from calcium carbonate. Since in the evaporation of sea water of the present content of salts and under the present conditions calcium carbonate is precipitated before calcium sulphate (gypsum), and since with a constant supply of fresh sea water calcium carbonate would be precipitated continuously with the gypsum, various hypotheses were suggested to account for the obvious removal of the calcium carbonate from the brine during its evaporation without its being deposited before or with the gypsum of these beds.

The present investigation is intended to apply the laws of equilibrium in salt solutions to this problem. The solubility of calcium carbonate at a given temperature is primarily a function of the

atmospheric content of carbon dioxide and of the concentration of calcium ions in the solution. The solubility of gypsum is a function of the concentrations of calcium ions and sulphate ions. By a comparison of the solubility curves of the two salts as affected by these variable factors, it is possible that a point may be found where the curves intersect in a way which might permit a reversal of the usual order of precipitation. If such a point is found, it would throw light on the condition of the atmosphere and of the surface of the earth at the geological period involved. The necessary calculations will be restricted at present to solutions of the ions in question, and may later be made to include the other ordinary constituents of sea water.

Three of the papers, contemplated as preliminary reports on the foregoing studies, are partially prepared and might be speedily completed were not deliberation and a review of the grounds involved in these complex themes more important than early production.

CHICAGO, *September 14, 1903.*

ARCHEOLOGICAL AND PHYSICO-GEOGRAPHICAL RECONNAISSANCE IN TURKESTAN

REPORT BY RAPHAEL PUMPELLY.

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At the end of 1902 the Carnegie Institution voted a grant to me "for the purpose of making, during the year 1903, a preliminary examination of the Trans-Caspian region, and of collecting and arranging all available existing information necessary in organizing the further investigation of the past and present physico-geographical conditions and archeological remains of the region."

The investigation was proposed because (1) there is a growing belief that central Asia was the region in which the great civilizations of the far East and of the West had their origins; and (2) because of the supposed occurrence in that region, in prehistoric times, of great changes in climate, resulting in the formation and recession of an extensive Asian Mediterranean, of which the Aral, Caspian, and Black seas are the principal remnants.

It had long seemed to me that a study of Central-Asian archeology would probably yield important evidence in the genealogy of the great civilizations and of at least several of the dominant races, and that a parallel study of the traces of physical changes during Quaternary time might show some coincidence between the phases of social evolution and the changes in environment; further, that it might be possible to correlate the physical and human records and thus furnish a contribution to the time scale of recent geology.

At my request Professor W. M. Davis assumed charge of the physico-geographical part of the preliminary reconnaissance.

Itinerary.

I left Boston March 18, accompanied by Mr. R. W. Pumpelly as assistant, and stopping over at London, Paris, and Berlin, reached St. Petersburg on April 23. There I had to remain several weeks to perfect arrangements and obtain the papers necessary for an extended journey in Turkestan. On May 15 we left St. Petersburg with an interpreter, and having been joined at Baku by Professor Davis and Mr. E. Huntington, a research assistant of the Carnegie Institution, we crossed the Caspian.

I found throughout our stay in Turkestan that orders had been sent from St. Petersburg to assist the expedition in all ways, and everything was done to facilitate the work. Prince Hilkof's orders obtained for us the continuous use of a car throughout our stay in Turkestan.

While I became deeply indebted to the general hospitality of all with whom we came in contact, I am under especial obligation to several gentlemen to whose ready assistance the expedition owes much of its success. From their excellencies Count Cassini and the Hon. Joseph H. Choate, Assistant Secretary of State Mr. Herbert Pierce, and Baron von Richthofen I received valuable letters to St. Petersburg. There, from His Excellency Mr. Semenof, vice president of the Imperial Geographical Society, I had letters of the first importance to high authorities in Turkestan, as well as from Generals Stubendorf and Artemonof. Valuable assistance was rendered by Mr. McCormick, our ambassador, and Mr. Ridler, secretary of the embassy.

Their Excellencies Prince Hilkof, Minister of Ways and Communications; Mr. Plehve, Minister of the Interior, and Mr. Yermolof, Minister of Agriculture, gave me circular letters to all the employes of their departments; while from the office of the Minister of War, who has control of Turkestan, orders were telegraphed to extend any desired aid to the members of the expedition. My plans were also cordially furthered by the Imperial Academy of Sciences at St. Petersburg, which passed a resolution asking the Minister of the Interior to facilitate our journey; by Mr. Karpinsky, then director of the Imperial Geological Survey; Professor Schmidt, and Mr. Bog-

danovitch, and by Mr. Chernachef, now director of the Imperial Russian Geological Survey.

In Turkestan we enjoyed the hospitality and assistance of their Excellencies the Governor General and Madame Ivanof; General Medinsky, governor of Samarkand; General Nalifkin, vice governor of Fergana, and Madame Nalifkin; General Oussakovsky, governor of Trans Caspia; Colonel and Madame Volkovnikof, local governor of Krasnovodsk; Colonel Kukol-Yasnopolski, governor of Ashkabad; General Ulianin, director of the Trans-Caspian railway; General Poslovsky, and General Gedienof. I owe the success of our Pamir expedition chiefly to the active interest and help of Colonel Zaitza, governor of Osh. To Baron Cherkasof, political agent at Bokhara, I owe much for his kindness during my visit to that place. At Old Merv we were entertained with great hospitality by Mr. Dubosof, superintendent of the imperial estate.

Using the railroad as a base and having horses and escorts wherever needed, we made flying excursions to many points, at different distances from the railroad, both in going and coming.

From Ashkabad we made an excursion across the mountains of Khorassan into Persia, accompanied by Mr. Yanchevetzki, secretary of the governor, and his intimate acquaintance with the water problems and with the country from the Aral south was of great use to us. On our return to Ashkabad we were joined by Mr. Richard Norton, who accompanied me throughout the journey.

The next stop was at Old Merv, where we spent several days among the extensive mines. Thence passing by Bokhara and making only a preliminary visit to Samarkand, we went to Tashkend, the residence of the governor general of Turkestan. Here the party divided, Professor Davis and Mr. Huntington going eastward to Issikul, where, after a month of joint work, they separated, Mr. Davis returning to America via Omsk and St. Petersburg, and Mr. Huntington going on to Kashgar.

After Tashkend, I visited Margelan and Andijan, the end of the railroad.

Continuing our journey to Osh, at the entrance to the mountain region, we organized an expedition to the Pamir, with the courteous aid of its governor, Colonel Zaitza. The way to the Pamir covered part of the route and two of the passes, the Terek and Taldik, in one of the great currents of ancient trade between China and western Asia, and it promised light on the physico-geographical part of our problem. After returning from the Pamir we visited the ruins

of Ak-si, in the northern part of Khokand, beyond the Sirdaria and examined the ruined sites of Samarkand, and of Paikend in Bokhara and a trenched tumulus at Annau near Ashkabad.

Throughout the journey, both by rail and in the side excursions, we had occasion to note the existence and position of a great number of former sites of occupation, both towns and tumuli.

It had been my wish to examine Balkh, the site of ancient Bactra, and other ruins of northern Afghanistan, but this was found to be impossible on account of the hostile attitude of the Afghans toward even Russians.

Outline Sketch of the Region.

A glance at a map of the Eurasian continent shows that the three seas, the Aral, Caspian, and Black, occupy parts of one great basin, bounded on the south and east by great mountains, and on the north by the Aral-Arctic divide.

If the Bosphorus were closed and there should exist a continued excess of rainfall over evaporation, these seas would merge and the basin would fill till it overflowed into the Northern ocean. The area of this Asian Mediterranean would be determined by the height of the northern divide, which is as yet unknown. In any event, it would be sufficient to submerge a large part of southern Russia and much of Russian Turkestan.

If, on the other hand, there should be a continued increase of excess of evaporation, the seas would dry up; the whole basin would be transformed into a vast desert, on the borders of which the retreating river mouths would be lost in the sands. Turkestan, once largely covered by water, is now in a state approaching this condition of aridity. The greater basin is broken up into smaller, disconnected ones, of which only one, the Black sea, has an outlet. The Aral stands 159 feet above the ocean, the Black sea practically at ocean level, the Caspian 84 feet below ocean level.

The great Volga and several small streams reach the Caspian; east of the Caspian only two rivers, the Sir and Amu (Jaxartes and Oxus), reach the Aral; and they gather water only at their sources in snow clad mountains; all other streams are consumed by direct evaporation and irrigation and have short courses, ending in desert sands.

According to Schwartz, about three quarters of all this vast region is desert and one quarter is capable of supporting the herds of the

nomads. Water can be distributed on about 2 per cent of the entire area, on land free from drifted sands. Along the base of the southern mountains stretches a chain of narrow oases at the mouths of the mountain valleys; there are other very narrow strips along the larger river courses, and more extensive areas inclosed between the projecting spurs of the eastern mountains; all the rest of the basin has become the prey of the moving sands, which are still very slowly but surely invading the oases. The boundary is sharply defined; within it is high cultivation; beyond is a sea of waves of sand.

As they extend eastward the southern mountains increase in height, till both they and the great spurs of the Tienshan—giant snow and ice covered crests and peaks—dominate the oases which are the offspring of their waters. It is on this mountain snow and ice that the life of the whole region is and has been from a remote period absolutely dependent.

This life is also limited by another factor—itself a result of the desiccation—the moving sands. For, other things remaining equal, while the shrinkage of the water areas can continue only till equilibrium between supply and evaporation is reached, and while there might be also cyclical periods of revivifying afflux, these compensations are offset in the oases by the steadily overwhelming progress of the sands.

The progressive desiccation of Turkestan is shown by direct observations during the past century, by artificial landmarks, by historical statements, and by natural records. The Aibughir gulf of the Aral was 133 kilometers long and 3,500 square kilometers in area in 1842, and dry land in 1872.

The volume of the Sirdaria has diminished greatly, as shown by the remains of old irrigating canals along its whole lower course, which are now too high to receive water. The statements of Arabian writers show that, within recent historical times, there was a far more numerous population than the country could support now, when all available water is utilized. Old water level lines occur at various heights up to 225 feet above the Aral.

The progress is not uniform, but is broken by periods of temporarily increased precipitation. Dorandt measured in 1874–75 a fall of 70 millimeters in the year in the Aral sea. Schultz, in comparing his surveys of 1880 with earlier maps, found a lowering of the level of 38 centimeters in nine years. On the other hand, Berg in 1901, comparing the gage established by Tillo, found the level 121

centimeters higher than in 1874. He calculates the total rise between 1882 and 1901 to be at least 3 meters, or 178 millimeters, yearly.

Judging from my observations and from those of others, especially of the Arabian writers and of the later Russian explorers, it would seem that the country has long been an interior region, dependent mainly on the snows and glaciers of the mountains for its life; that there have been within the present geological period great fluctuations in the amount of water derived from the mountains as recorded in high and low shore lines of the seas, and in the strata left by different expansions of the united waters of the Aral and Caspian and containing living forms; that man already existed within the region during at least the last great maximum of moisture.

Evidences of Former Occupation.

In our earliest historical records we find the country occupied as now by dwellers in numerous cities, surrounded by deserts in which lived nomad peoples. The town dwellers seem to have been at least largely of Aryan stock and the nomads of Turanian.

Who were the contemporaneous and the successive dwellers in the many towns? To what different races may they have belonged? Whence did they come into the land? What were their civilizations and what their relations to other civilizations and to those of the modern world? These are our questions, and they can be answered only to a greater or less extent by a study of the results of excavation and in the concentrated light of comparative science in archeology, ethnology, and language and of survivals in arts and customs, for the answers to some of these questions will be found rooted deep in the human strata of the ancient world. Asia abounds in the fragmentary survivals of stocks, arts, customs, and languages.

The vestiges of former occupation by man are varied in character—in the eastern mountains pictographic inscriptions recalling those of American aborigines, some rock sculpturing, and rough stone idols. At Lake Issikul Professor Davis describes stone circles, recalling some of the dolmen-like forms, and submerged masonry in the lake.

Along the river courses are abandoned canals which can no longer be supplied with water, and the Russian maps abound in indications of ruined towns, "forts," etc. The most important remains are the tumuli and the town sites.

TUMULI (OR KURGANS).

The tumuli proper are accumulations of earth, of rounded generally symmetrical form, often more or less elliptical in horizontal section. We met with them first along the base of the mountains east of the Caspian, but I saw none at a lower elevation than 250 feet above that sea. From this point eastward they abounded, with some interruptions, as far as to near Andijan. Generally they were large—100 to 200 feet long and 30 to 50 feet high. They are much more abundant east of the Oxus than to the west. At one point I counted fifteen in sight at once. Besides these larger tumuli, there are, especially along the Sirdaria in Fergana, localities with a great number of small mounds a few yards only in diameter, suggesting burial after battles.

Mounds more or less resembling the larger ones are described by De Morgan at points in northern Persia, and they occur through southern Siberia and on the plains of southern Russia and of Hungary. In all these countries they probably have different origins—different reasons for their existence. Those in Siberia and on the Black sea have been extensively excavated.

There has been some unsatisfactory excavation of those in Turkestan, mostly with unrecorded results. The kurgan at Annau, near Ashkabad, was trenched some years ago by General Komorof. This afforded the best exposure of internal structure. It is nearly 200 feet long and 35 feet high and slightly elliptical in horizontal section. It consists of fine, horizontally stratified layers of made earth. Layers of silt and broken cobbles alternate with layers rich in gray ashes and charcoal, and others of closely matted fragments of pottery. Animal bones, teeth, and jaws, some of which are partially calcined, occur frequently in all layers, with a few human bones and skulls. Several whole vases and muffle shaped chests, made of coarse pottery mixed with dung, had been cut by the trench. These appeared to contain only fine ashes and charcoal. Most of the fragmentary pottery is of this coarse, dung mixed quality, but there are also many fragments of finer texture, decorated with sample designs of black on red, even at the bottom of the trench. We found several granite stones with curved plane surface which had evidently been shaped for mealing grain by the *metate* method, and also a roughly spherical stone that had been pierced, apparently for the insertion of a handle and to use as a maul. Gen-

eral Komorof found one celt of quartzite and some needles of bone, but absolutely no metal. Of the bones, I sent a representative collection to Professor Zittel in Munich, for determination.

The whole character of the tumulus shows that it grew from the plain upwards, as a slow accumulation of the debris of long occupation. The fact that the layers, even at the top, extend horizontally to the edges proves that it was formerly flat topped and much larger, for had it during occupation ever assumed a spherical surface the growth would have been in concentric layers. The same reasoning would show that it was never abandoned for a long time and again occupied. Since its surface has not been gullied, it seems possible that it was shaped by wind action, although the earth is somewhat firmly cemented. A further indication of antiquity is the present condition of the granite grain grinders, now rotten and crumbling.

One peculiar feature in the structure is the interruption and bending over of the layers at the two apparent earth walls.

Several other kurgans that we examined, which had been partially cut away for brick making, etc., and some of which were much larger and higher, showed the same horizontal stratification of earth, burnt earth, ashes, charcoal, and fragments of bones and of pottery. In the upper part of some of these we observed traces of walls of unburned bricks. The only artifacts found in these were the simplest form of flat stone for grinding grain (like those found in the Annau kurgan) and some flat stones, each with a hole drilled wholly or partially through it from both sides.

Ancient Towns.

The absence of easily obtainable stone throughout the lowlands of Turkestan determined the use, almost exclusively, of clay, both unburned and burned, in construction. Unburned clay predominated immensely, used both as sun dried bricks and in heavy layers of raw clay. In consequence of this, all ruins older than a late Mussulman period are represented only by accumulations of earth filled with broken pottery and fragments of burned bricks. These accumulations are flat topped mounds, ranging up to a square mile or more in area and from 15 to 20 feet upward in height, and in places, as at Merv, occurring in groups covering many square miles. They occur within areas in which now, or formerly, water was accessible, and are found also more or less buried in sands beyond

the mouths of the retreating rivers, in places once fertile and now desolate.

Ruins near Atrek River.—A type of regional desolation and abandonment is in the territory between the lower Atrek and the Caspian. Here, over an area of many square miles, are the ruins of cities, 30 or 40 miles from the river Atrek, the nearest water, and in the heart of the desert. The remains of canals show that the cities were watered from the Atrek, but this river now lies too low to feed the canals.

Ancient Merv.—The ruins of ancient Merv are said to cover about 30 square miles and consist of several cities of different ages. Two of these—the Ghiaour Kala and the Iskender Kala appear to be the more ancient. The remains of a circular wall extend, with a radius of about four miles, all around these several cities. To judge from its degraded condition, it may possibly represent a very ancient enclosure within which diminishing populations have rebuilt after successive destructions by war. Merv existed in remote antiquity and is one of the cities mentioned in the Zend Avesta.

The walls of Ghiaour Kala, though now reduced to a hillocky ridge perhaps 50 or 60 feet high, enclose plateaux, 20 or 30 or more feet high, of accumulated debris. From these walls we could see far away on the northern horizon, in the desert, other flat topped mounds apparently of great height and extent.

Ruins of Paikend.—The ruins of Paikend represent the type of cities abandoned for lack of water and then buried by the progressing desert sands. It was a great center of wealth and of commerce between China and the west and south till in the early centuries of our era. The recession of the lower ends of the Zeraffshan river brought its doom. Now only its citadel mound and the top of parts of its wall rise above the waves of the invading sands.

Samarkand.—Next to those of Merv the ruins of Samarkand are the most extensive. Its position must have made it an important center of commerce and wealth probably throughout the whole period of prehistoric occupation, as it has been during historic times. Situated in the heart of the very fertile oasis of the Zeraffshan river, it lies also on the most open and easiest caravan routes connecting China and eastern Turkestan with Afghanistan, India, and Persia.

Samarkand has, even within the past two thousand years, been sacked, destroyed, and rebuilt many times. Like Merv, its rebuildings have often been on adjoining sites, and the determining of the whole area covered by these various sites remains to be made. There

is evidence that it is very extensive. The most ancient seems to be the plateau or "tell" called "Afrosiab," to which tradition assigns the site of the Samarkand Maracanda of Alexander the Great. This is a plateau of "made earth," the debris of ruins, standing on the "loess" plain. It is covered to a great extent with Mohammedan cemeteries, and some traces of Mussulman occupation, and with fragments of pottery and of bricks. The loess plain is deeply dissected by a stream, and several gullies have been cut in both the plateau of the ruins and the loess. It is difficult to distinguish between the "made earth" of the plateau and the underlying "loess," except through the presence of fragments of pottery, charcoal, and bones.

We found such fragments down to a depth of about 40 feet below the general surface, in the gullies, and it is not improbable that the thickness of debris is still greater. Above this general surface rises the citadel mound to an additional height of 30 to 40 feet, or 170 feet above the stream at its base. Judging from the excellent topographical map of Afrosiab, of the general staff, the loess plain lies about 50 feet above the stream. This would make it possible that the citadel mound represents an accumulation of over 100 feet of debris. The surface of the rest of Afrosiab is very irregular. While in general it ranges from 100 to 140 feet above the stream, there are numerous depressions, the bottoms of which are level plains, 150 to 300 feet in diameter, standing 70 to 80 feet above the stream.

The general arrangement of these depressions is such that if filled with water they would form a connected, irregular system of ponds; and there is a channel about 100 feet wide which starts in high up on the cliffs overhanging the stream, and, traversing Afrosiab, opens out again on to the stream valley, after communicating with most of the depressions. It all suggests a former water-system, but it is one that it would seem could have been effective only if the stream ran at a considerably higher level than at present. The large scale map of the district shows this stream to diverge from the Serafschan in the same manner as many irrigating canals, and to run with a lower grade than the parent river, the river having a grade of about 20 feet per verst as against about 7 feet in the derivative stream or canal. Judging from these facts it seems not impossible that this stream was originally a canal supplying the city, and that it has in the course of ages cut its channel deeper in the "loess."

The former walls of the city are represented now by ridges rising

20 or 30 feet above the surface within. Where the walls are cut by gullies old galleries are exposed which seem to have been continuous with the wall. Quintus Curtius states 70 stadia as the extent of the walls in the time of Alexander. This, if the short stadia were meant, would be about three miles, which would be approximately the circumference of that part of Samarkand now called Afrosiab.

As in all Turkestan, so at Samarkand, the older structures still standing are those of the Mohammedan period. The many immense and wonderfully decorated mosques built by Tamerlane, though now falling into ruin, belong among the wonders of the world; and this not only on account of their great size, but also because of the beauty of their decoration. Seen from Afrosiab, these ruins tower high above the rich foliage of the oasis city—evidences of the wealth of treasure that Tamerlane had accumulated in Turkestan within two centuries after Genghis Khan had sacked the country and massacred much of its population.

Review of the Field.

What I have been able to say here regarding the archeology of Russian Turkestan seems but a meager statement; but it was soon clear that all that could be accomplished in such a reconnaissance would be the observation of the character and abundance of the evidences of former occupation, and to obtain some idea of their distribution and size.

Our reconnaissances covered a territory nearly 1,400 miles long. It was necessarily only of a preliminary character, and intended to supply a general idea of the problems to be solved and of the best points at which to begin.

While we have been surprised at the abundance of the data offered by the region toward these solutions in natural and artificial records, we are impressed with a realization of the intimate relation in which this region stands to the Quaternary and prehistoric history of the whole continent. Physically it forms part of the great interior region extending from the Mediterranean to Manchuria, whose history has been one of progressive desiccation, but in Russian Turkestan the effects of this have been mitigated by the snows of the lofty ranges and the lower altitude of the plains.

Archeologically this region has, through a long period, been a center of production and commerce, connecting the eastern, western,

and southern nations, and accumulating wealth that has made it repeatedly the prey of invading armies. It has been from remote time the field of contact and contest between the Turanian and Aryan stocks; but its problems, both physical and archeological, are parts of the greater problem underlying the study of the development of man and his civilization on the great continent and of the environment conditioning that development.

The many fragmentary peoples surviving in the remote corners and in the protected mountain fastnesses of Asia, preserving different languages, arts, and customs, indicate a very remote period of racial differentiation, with subsequent long periods for separate development. They point also to the long periods of unrest and battling in which the survivors of the vanquished were forced into their present refuges. And this unrest was probably the remote prototype of that which in later prehistoric and historic time sent out its waves from the Aralo-Caspian basin. It was probably from the beginning a condition in which the slowly progressive change toward aridity in interior Asia was ever forcing emigration outward, displacing other peoples, and thus working against the establishment of a stable equilibrium of population.

Asia is thus the field for applying all the comparative sciences that relate to the history of man. The materials lie in cave deposits, in rock pictographs, in tumuli, dolmens, and ruined towns, in languages, customs, religions, design patterns, and anthropological measurements.

Turkestan, from its geographical position, must have been the stage on which the drama of Asiatic life was epitomized through all these ages of ferment. Races and civilizations appeared and disappeared, leaving their records buried in ashes and earth; but the fertility of the soil produced wealth, and the position kept it ever a commercial center.

So far as our problems of archeology and physical geography are concerned, Turkestan is practically a virgin field. In geology and cartography the Russians have done a surprising amount of excellent work; but the modern methods of physico-geographic study have not been applied, and the little archeological work done has been in the nature of hunting for curios and treasure. Throughout southern Siberia tumuli have yielded up vast treasures in the form of gold ornaments dating from various epochs. Scientific excavation has been undertaken only in southern Russia, in the Caucasus, and in Persia.

In Persia, M. J. de Morgan has for several years been conducting a thoroughly scientific investigation at several points, and especially at Susa, where he has already obtained results of the greatest interest. The acropolis of Susa is 105 feet high. M. de Morgan's preliminary tunnels, run into the hill at different levels, showed it to be composed of made earth from the base upward. Stone implements and pottery abounded up to 36 feet from the top. The pottery improved from below up, and among the fragments he recognized a variety belonging to a group peculiar to Egypt, Syria, Cyprus, and most of Asia Minor, but not known from Mesopotamia. De Morgan had found this in predynastic tombs in Egypt, and ascribed it to a period before the eightieth century B. C. At 45 feet below the top he found tablets and cylinders with hieroglyphic inscriptions which Scheil considers as belonging before the fortieth century B. C.

M. de Morgan asks: "If the refined civilizations of the past 6,000 years, with their great structures and fortifications, have left only 45 feet of debris, how many centuries must it have required to accumulate the lower 60 feet when man used more simple materials in the construction of his abodes?"

East of Russian Turkestan excavations have been recently made by Stein in some cities in the Tarim basin, which we know from Chinese history were buried by sand in the early centuries of our era.

The thickness of made earth in the tumuli and town sites of Turkestan is sufficient to give reason for expecting evidences of very long continued occupation. The dryness of the climate makes possible the preservation of any traces of written or incised documents that may have existed. Excavation conducted with the idea that everything met with—the earth itself, the character, position, and association of fragments—is part of history, cannot fail to be most fruitful in results.

It was in all probability from Turkestan that the earliest products of metallurgy in bronze and iron successively progressed to the western world—a progress that in each case carried with it a revolution in civilizations. We do not know whether this region saw the birth of the metallurgy of those elemental substances which, beginning with copper and tin and progressing through bronze to iron and steel and the use of coal, marks the birth of civilization and its great revolutions. If it was not the birthplace of this art, and if it was a distributing center, it is a long step nearer to the source, whether this was China, East Turkestan, or India.

RECOMMENDATIONS.

Since Turkestan is under the control of the Minister of War and much of its frontier is closed to travelers, it is necessary to have the permission and good will of the government in order to pursue investigations. To inaugurate any extensive plan of archeological excavations will require tactful negotiation at St. Petersburg. I have good reasons for believing that the desired concessions can be had on a basis of division of objects found, and with a sufficient time allowance for the study of all the material. Such a plan should include both town sites and large and small tumuli. Of the town sites I would recommend, as points to begin on, in the order stated.

Town sites.—Afrosiab (Samarkand), Ghiaour Kala (Old Merv), Paikend (west of Bokhara), Aksi (on the Sirdaria). The high ruins seen several miles to the north of Ghiaour Kala. A very high one seen from the railroad a few miles west of the Aum-daria.

Tumuli.—Both the tumulus mentioned at Annau, near Ashkabad, and another lying a short distance from it. Others west of Ashkabad, north of Old Merv, near Djizak; also many of the mounds of small size which seem to have a different significance.

As bearing on the age of the tumuli, it is important that the relation of the base of the mound to the surrounding earth be studied to determine by how much, if any, the level of the plain has been built up since the first occupation of the site, and to see also by how much the mound has shrunken in size at its base, as it certainly has n horizontal section at the top. In connection with the question of age of the tumuli and in relation to the last expansion of the Aralo-Caspian seas, it would be very desirable to determine the lower altitude limit of distribution. I did not see any below 250 feet above the Caspian.

Similar observations are needed on the west coast of the Caspian, where De Morgan found no antiquities on the lowlands in the Lenkoran country, but at a higher level abundant tombs of the bronze period and of the transition to iron.

As further connected with the relation of human occupation to the formerly expanded water area, there is needed a determination of the altitudes of the Manytsch divide between the Caspian and the Black sea, and of that between the Aral and the Arctic ocean. Both of these are now not far from railroad bases.

Results in Physical Geography.

Both our own observations and the excellent and extensive work of the Russian geologists show that the progressive desiccation of the region has greatly diminished both the area of cultivable land and the volume of water, and greatly reduced the population. Is this change a phase of cyclical phenomena—of cycles of long periodicity? In what relation have the geologically recent secular phenomena in central Asia stood to man and civilization in that region and to the outside world?

One of the chief objects of the reconnaissance of the past season was to determine whether a systematic investigation would be likely to throw light on these questions. Perhaps the most important result is our finding that successive physical events have left such abundant records, written in large strokes, all over the mountains and the plains.

The work of this year has not only made a most promising beginning in this interpretation, but has shown that it is probably possible to correlate the different events among themselves and with the period of human occupation and possibly with similar physical events in Europe.

As an interior region, central Asia is arid and dependent for its water almost wholly on its bordering mountains. It is also self contained—*i. e.*, without drainage to the ocean. Changes of climate, resulting in great fluctuations of water supply, would therefore probably be recorded by old shorelines at different levels. They might also be more or less legibly recorded in the evidences of repeated glaciation and erosion in the high mountains.

Professor Davis has found traces of an old shoreline about 600 feet above the west shore of the Caspian sea, and a very distinctly marked one on the east side, at an elevation of 200 feet or more. Further search for shorelines was left to form the object of a more extended special study than could be made in our general reconnaissance.

In the Eastern mountains, near Issikul and Sonkul, Professor Davis found clear evidence of two and probably three glacial epochs. Mr. Ellsworth Huntington, working in Kashgaria, found proof of three epochs, and later, of five in the successive moraines of a large number of glaciers studied by him in the Alai mountains. Between some at least of these there were long interglacial intervals.

Mr. Huntington reports records of climatic oscillations shown, not only in these moraines, but also in the valley terraces and erosions, and considers them members of a group of sympathetic glacial phenomena.

Professor Davis noted along the northern edge of the Kopet-dagh, the mountains bordering the plains east of the Caspian sea, and in the Eastern mountains, evidence of a longitudinal dislocation, accompanied by great block uplifts, formed apparently after the wearing down of the mountain masses to a peneplain and preceding an active dissection of the elevated mass. This dislocation had been already observed by Muschketof, who states that it extends far along the edge of the Kopet-dagh.

These block uplifts, by lowering the baselevel, caused a remodeling of the mountains, and have left their record on the lowland plains, which they have helped to create, by the vast amount of material poured out on them by the eroding streams.

The block uplifting and the tilting being correlated with the growth of the alluvial Ferghana lowlands, and the relation of the glacial expansions to the valley cuttings in the Trans-Alai range being clearly recorded, it becomes a matter of great interest to correlate these Quaternary events of the Trans-Alai valleys with those of the Alai, and the growth of the plains with the progress of human occupation.

Mr. R. W. Pumpelly studied independently a profile from the Sirdaria southward across the two mighty snow and ice ranges, the Alai and Trans-Alai. He found clear evidence of two long separated glacial epochs recorded in extensive moraines and on the Pamir in apparently corresponding high shorelines around Lake Karakul. These glacial epochs he has correlated with orogenic movements of the Trans-Alai, there being a definite relation between the glacial trough bottoms of the two epochs and the present stream floors. Of the Alai range, he found that there had been a block uplift followed by a block tilt, both with a dislocation through the border of the lowland plains to the north, and leaving their records in alluvium capped hills and terraces along the valley sides and in the dragging up or tilting of the fluvial sediments or river " fans " on the lowland borders. These movements he has correlated with the glacial geology, making the block tilt an interglacial event.

It is not impossible that, by extending the study of glacial records from the Central Asian ranges through the Elburg and Caucasus, it may be practicable to correlate Asiatic and Alpine glacial events ;

and since the great basin was fed both by glaciers of the southern ranges and by the great ice cap of Russia, a correlation of both might be effected; for, in view of the great orogenic movements to which the Caucasus, the Persian, and the Tienshan have been subjected, it cannot be positively asserted that the Central Asian glacial expansions were all contemporaneous with phases of the mundane glacial epoch.

As regards further work in physical geography, Professor Davis writes:

"The order in which I should like to see the * * * studies taken up, in order to most rapidly define the conditions of early human history, on the plains is as follows:

"The shore lines of the Caspian and Aral seas; first on the southwest, south, and southeast, then on the northeast and the associated plains.

"The double belt of piedmont plains and bordering ranges with special work in certain glaciated valleys.

"The deposits of loess from Samarkand to Tashkend.

"The Issikul basin. By a special, independent party.

"Secondarily, Block mountains and the Narin formation."

SUMMARY.

We have shown that the recent physical history of the region is legibly recorded in glacial sculpture and moraines, in orogenic movements in valley cutting and terracings, in lake expansions, and in the building up of the plains, and we have made some progress in correlating these events.

We have also found full confirmation of the statements as to a progressive desiccation of the region of long standing, which has from a remote period continually converted cultivable lands into deserts and buried cities in sands.

We have found, widely distributed, great and small abandoned sites of human occupation, with evidences of great antiquity.

We have reason to think that a correlation of these physical and human events may be obtained through continuance of the investigation, and that archeological excavations will throw light on the origins of Western and Eastern nations and civilization.

APPENDIX

ESTIMATES SUBMITTED BY ADVISORY COMMITTEE ON ANTHROPOLOGY

The estimates submitted by the Advisory Committee on Anthropology were omitted from the report printed in YEAR BOOK NO. 1, pages 174 to 181. To make the report complete they are here given:

I. Physical Anthropology:		
I. Purchase of apparatus	$1,500	
II. Annual expenses:		
Salaries of two specialists	3,600	
Two observers, at $600	1,200	
Four computers, at $600	2,400	
Stationery, transportation of instruments, postage, etc.	300	
		$9,000
II. Archeology:		
Salary of specialist	$1,800	
Employment of field assistants and labor	1,500	
Traveling expenses	1,500	
Identification of fossil remains	500	
		5,300
III. Ethnology:		
Salary of specialist	1,500	
Purchase of specimens and traveling expenses, clerical work, etc.	3,000	
		4,500
Salary of specialist	$1,200	
Field work	800	
Publication	800	
		2,800
IV. Publication:		
American Anthropologist		1,500
Total grants suggested		$23,100

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